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VOL. LXIX, NO. 3 SEPTEMBER 1949

He can see a thousand miles

ALARM · INDICATOR RECORD

TIME RECEIVED A P BY
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	SYNCHRONIZATION - START					CA FAIL SEND CKT.	STATION IDENTIFICATION
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	64 KC PILOT ALARM AT NON-SW. MAIN								3096 (SP. LINE) PILOT AT SW. MAIN			
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THE
SCIENTIFIC MONTHLY

SEPTEMBER 1949

MAKAPANSGAT

GEORGE B. BARBOUR

Dr. Barbour (Ph.D., Columbia, 1929) is professor of geology and dean of the McMicken College of Liberal Arts at the University of Cincinnati, and has just concluded a term as president of the Ohio Academy of Science. Born in Scotland and educated in the universities of Great Britain, Germany, and the United States, he spent twelve years in China, where he was associated with Davidson Black, P. Teilhard de Chardin, W. C. Pei, and Franz Weidenreich in the Peking Man discoveries. He was geologist with the University of California Expedition to Africa in 1947–48 and has also traveled extensively in Europe, Siberia, Southeastern Asia, and the Caucasus.

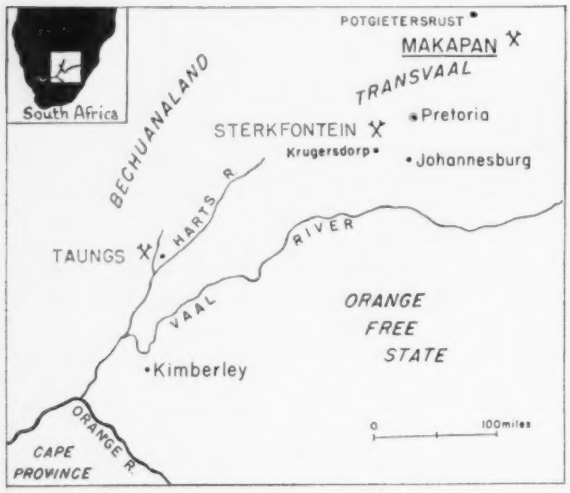
MORE significant light on human prehistory has come from a secluded valley in the northern Transvaal, 125 miles from Pretoria, than from any other single locality in South Africa. Caverns in the hillside have yielded proof that they were inhabited not only by men of the Old and Middle Stone Ages, but also by creatures so primitive that they span the gap between undoubted apes and true men. These exciting discoveries have been almost disconcerting.

From the capital city, the Cape-to-Cairo highway strikes north across the Bushveld along the old caravan route taken by the early Dutch *Voortrekkers* in 1838, and then northeast around the edge of Springbok Flats to a gap in the mountains through which the Magalakwin flows to join the Limpopo. The stream has so low a gradient that after heavy rain it floods a strip 1,200 yards wide for 50 miles before entering the narrows. The settlement on the riverbank where the God-fearing pioneers outspanned their teams for the night still attests by its name—Nyl-stroom—their error in mistaking this broad flood for the headwaters of the Nile, the "River of Egypt" of which their Bibles had told them. Five miles beyond the entrance to the water gap, a tributary valley heads back into the limestone hills. Here, in reprisal for an attack on their camp, the Boers drove the native chief Makapaan and his marauding tribesmen into a cave, piled brushwood, faggots, and stones in the entry, and set light to the bonfire. Makapaan's men were virtually wiped out, and the Boer leader, Potgieter, was killed. For a century the Valley of Makapansgat and the town of Potgieters-rust have been famous landmarks.

I

The Historic Cave is only one of a number in the Transvaal Dolomite that flanks Makapansgat Valley. Some of the caverns were worked extensively over several decades for the thick layers of cave travertine with which most of them were floored at an early stage in their long history. Once these layers of high-grade lime had been extracted, the overlying mass of rock debris choking the caves made further excavation risky, and the miners kept moving on to new prospects, little suspecting the value of the precious rubble over their heads.

In 1925 Professor Raymond Dart noted that the breccia forming the roof of one of these caverns contained quantities of bone fragments. Some of them were blackened as if charred, and, when lab-



oratory analysis confirmed the presence of free carbon, Dart concluded that there seemed "to be little doubt from the evidence available that the bone bed is the 'kitchen-midden' result of human occupation at a remote epoch."* His critics suggested that the carbon might have been due rather to bush fires, blasting powder, or even to sightseers' bonfires. For more than twenty years Dart's daring conclusion was accepted with distinct coolness—or at least placed in cold storage.

In 1937 Dr. van Riet Lowe visited Makapansgat. In a miner's drift close to the Historic Cave he found that a huge block of roof rock had fallen to the ground. This proved to be a consolidated mass of limestone rubble, evidently an old cave filling, on the upper surface of which was a layer of broken bones and stone implements of advanced Paleolithic type, somewhat resembling the Acheul culture of Western Europe. Artifacts of this kind have been known to occur widespread throughout South Africa, but almost always on the ground surface, or in layers of superficial soil or river gravel—never before unquestionably on the very spot where their original owners had dropped them in a once-inhabited cave. The block of breccia had detached itself along the bone layer that had been an occupied site, and further collapses of this less coherent floor-earth showed by the color-banding and ashy layers that the cave had been inhabited repeatedly over a considerable period of time. It was appropriately referred to as the "Cave of Hearths."

* Bibliographic references to this and other problems relating to the various Australopithecinae sites will be found in a recently published paper, "Ape or Man?—An Incomplete Chapter of Human Ancestry from South Africa," Presidential Address, Ohio Academy of Science. *Ohio J. of Sci.*, 1949, 49, 129–46.

A hundred yards away, van Riet Lowe found another abandoned limeworkers' drift with hearth layers of even brighter color-banding, which earned it the name "Rainbow Cave." But the stone implements embedded in the floors of this site show a marked advance in technique, and belong to a Middle Stone Age flake industry known as the Pietersburg culture. Lance heads of this type recall the Solutrean of France, the general workmanship closely resembling the Levallois technique.

The importance of the locality could no longer be gainsaid, and the two sites were proclaimed a National Monument by the government. Thanks to a grant from the Bernard Price Foundation at the University of Witwatersrand, the Archaeological Survey was able to embark on a scientific excavation of the Cave of Hearths.

Shortly after arriving in the Transvaal as a member of the University of California's African Expedition under Dr. Charles L. Camp, I was invited to study and report on the prospects of success at the Makapan sites. It was felt that experience gained at the Choukoutien caves near Peking, where *Sinanthropus* was found, might be of value in laying plans for more extended sapping operations, and might also serve to encourage the directors in their hope that further excavation would yield even more important results. These hopes were borne out by the almost immediate exposure of two hand-axes of Chelles-Acheul type and shortly after by the recovery of a fractured human jaw of unmistakably Neanderthaloid type. The entire deposit had been consolidated by calcareous cement, so that the cave and its filling constitute a "sealed" site. There is thus no reason to doubt that the spent hearths and the Paleolithic tools belonged to men who once inhabited this South African cave and who resembled their relatives of the Old Stone Age north of the Mediterranean.

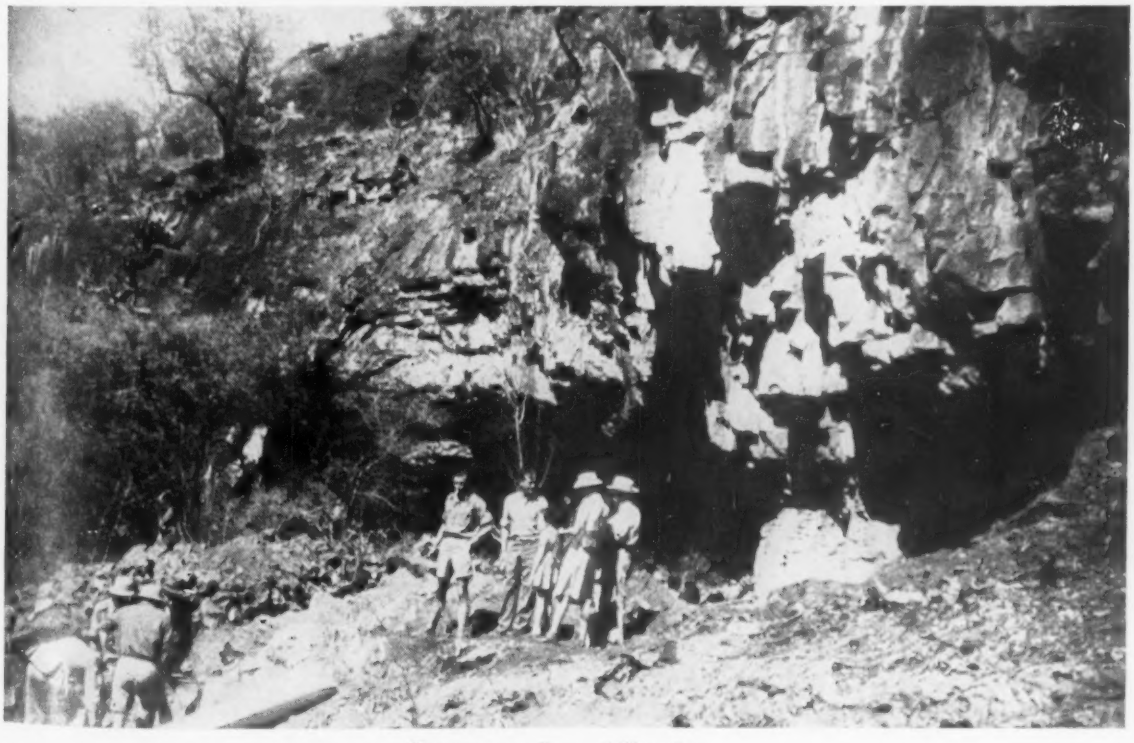
The Cave of Hearths, the Rainbow Cavern, and Makapan's Historic Cave are situated close together, high on the side of a valley that ends abruptly in a cliff-walled amphitheater three quarters of a mile to the east. Half a mile in the opposite direction, the westerly dip of the strata brings the soluble dolomite down to lower elevations, and the most extensive workings of all are only 50 feet above the level at which the alluvial floor of the valley abuts the base of the hillside. At this point, however, the contact is buried under an immense dump of waste rock. One of Professor Dart's photographs shows this Limeworks site

viewed from across the valley. Above eye level, beyond the upper road, are seen the sinkholes and cave openings by which access underground was first gained. It was in one of those to the left of the center that Dart's original find was made. Below the upper road are the dumps of debris from the caves—gray in the half tone, but red and yellow in the field. In the left center stand the deserted lime kilns, and beneath them a broad fan of white refuse from the kilns, approached from the right by the lower road. Between the native kraal of conical-roofed huts with their brushwood fence (extreme right, below) and the nearest corner of the refuse dump is the flat-topped sieving platform. Here more than six hundred tons of breccia have recently been broken up and carefully inspected for any trace of fossiliferous material worth transporting 150 miles to the laboratories of the Witwatersrand University in Johannesburg.

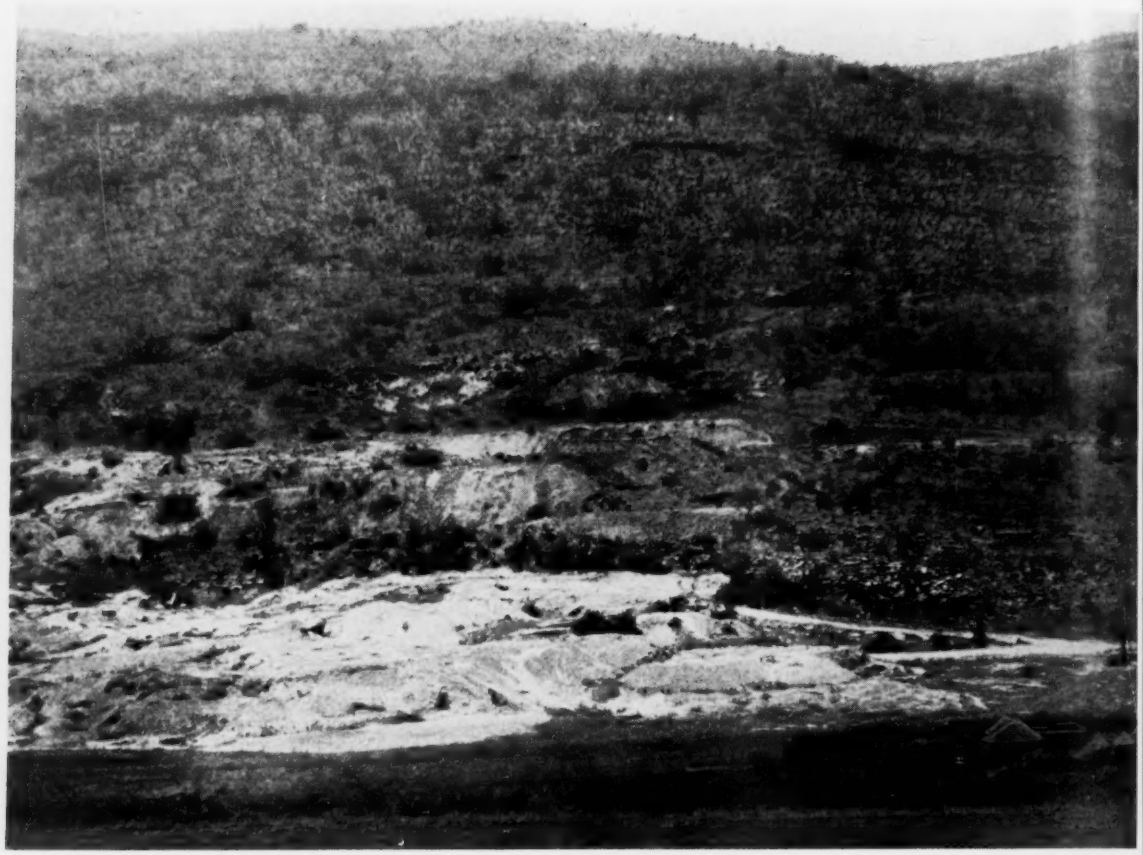
At the time of my visit in 1947 the higher caves were receiving the lion's share of attention, but I urged in my report that more intensive work also be done at the Limeworks. One afternoon when the native crew had knocked off work at the Cave of Hearths, Mr. James Kitchings, who was supervising the excavations, took a busman's holiday in the Limeworks Cave. He was rewarded by finding the damaged occipital bone of a primate which Dart recognized as of the same genus as the *Australopithecus africanus* described by him in 1926 from the Norlim quarries at Taungs 300 miles to the southwest. The Makapansgat individual, however, differed sufficiently in anatomical detail to warrant the establishment of a new species, and the earlier discovery of evidence of fire suggested the name *A. prometheus*, after the Greek hero who brought fire to man. Further excavation by E. R. Hughes and Kitchings revealed the almost perfect jaw of a twelve-year-old adolescent, an infant parietal, and part of the face of a female adult.

Hammers point to Old Stone Age artifacts in the Cave of Hearths.

The finds were all made in blocks of breccia from the "southern cavern," access to which is



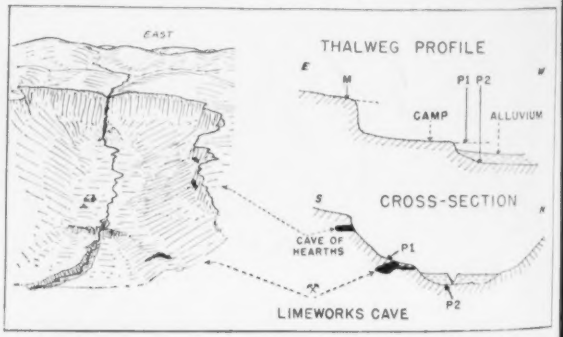
Entrance to Cave of Hearths.



Limeworks site. (Photo by Professor Raymond Dart.)

through the dark shaft seen at the right in the photograph. This is actually an artificial entrance opened by the limeworkers, who were following a six-foot seam of travertine down into the hillside. As in the other caves, the travertine layer of virtually pure lime carbonate once floored a cavern that later became choked with fallen rock and breccia. In the normal cycle of cave development, travertine forms during the stage when water is still percolating freely through the bedrock fissures and dripping from the ceilings, whereas the caves only become habitable by man or beast at a later, drier period; hence, the fossiliferous layers usually lie above the dripstone layers and beneath the zones of coarser collapsed roof rock. To the left of the black entrance hole can be seen the banded and distorted layers of sand and lime which normally overlie the travertine, and which represent successive floor levels of the original cavern.

In order to inspect the fossiliferous layers *in situ*, one must find a place from which the travertine layer has been removed, and then look up at the roof. I am indebted to Professor Dart for the photograph, which is the only good picture thus far secured of an exposure of this kind. It shows the *under* surface of a layer of bone breccia, with a patch of the cream-colored banded travertine still in place, forming the frieze of the gallery roof. Obviously, quarrying away more of the travertine seam would further sap the cave filling and might reveal a wealth of additional fossil material. In the meantime, more than twenty tons of fossiliferous breccia have gone from the dump to the laboratories at Johannesburg for study, and continuance



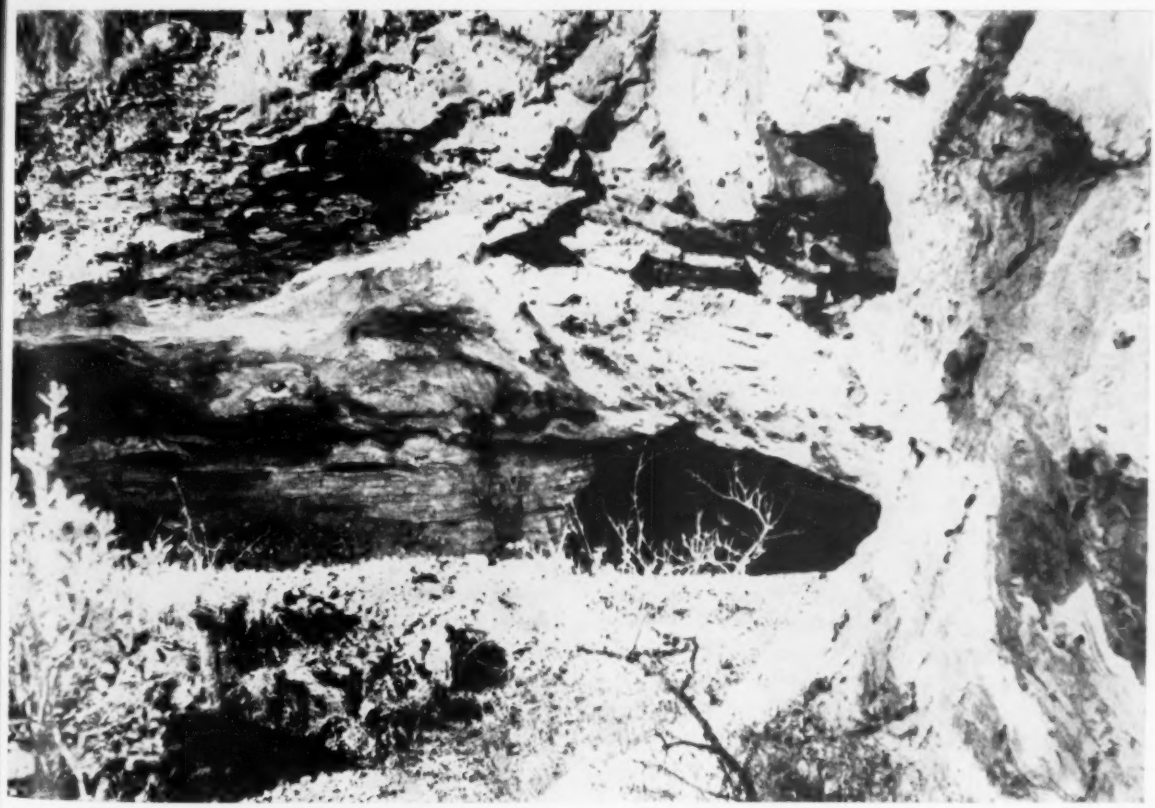
Makapansgat Valley. Relative position of erosion surfaces *M* (Miocene?), *P1* (Pliocene?), *P2* (Pleistocene). (Courtesy of *Ohio Journal of Science*.)

of the work is bound to yield further important results.

The special significance of the Limeworks discoveries lies not merely in what can now be determined regarding the nature and human status of the creatures who once inhabited these caves, but in the link they furnish between the various other sites from which Australopithecine remains have been recovered. Makapansgat throws light on some of the problems of the caves in the Krugersdorp area—Sterkfontein, where Broom's first discovery of *Plesianthropus* (1936) continues to be followed by further finds; the Kromdraai fissure, in which he found *Paranthropus* (1938 and 1948); the diggings at Bolts Farm, where the University of California Expedition unearthed further faunal material in 1947; and Taungs, the scene of Dart's original find and of recent excavations by the American scientists. The exact status of these various primates from the Transvaal has been a matter of keen debate. The Australopithecinae clearly belong to the same large group that includes both man and the anthropoids. Skull measurements show that their brain volume places them in the hitherto unfilled gap, or "No-man's-land," between the largest-brained apes and the smallest-skulled humans. The dentition is more human than apelike, and there are other anatomical details that appear to be manlike rather than anthropoid. In fact, LeGros Clark has gone so far as to say that there is nothing in their anatomy which definitely precludes the possibility of their being ancestral to man of the later Ice Age days.

II

The party of the University of California Expedition to which I was attached was unsuccessful in attempts to secure further Australopithecine material in any of the two dozen localities excavated in the vicinity of the original Taungs site. But Drs. Camp and Peabody found, in addition to important faunal remains, evidence of peculiar interest bearing on the predatory behavior of these remarkable primates, evidence which deserves special mention. At Makapan and the Krugersdorp localities, the remains of the Australopithecinae invariably occur along with skeletons and broken bones of other animals. Among the rather frequent fossil items are skulls of the baboons *Parapapio africanus, P. broomi,* and *P. jonesi.* Dart had been impressed with the number of these skulls that showed signs of having been smashed in from



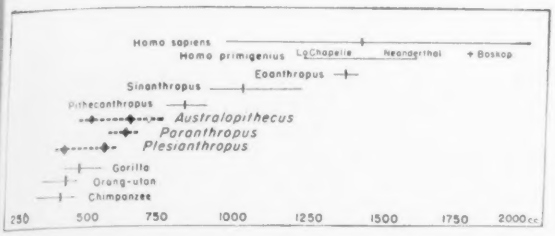
Entrance to "Southern Cavern," where *Australopithecus prometheus* was found. (Photo by Professor Dart.)

Breccia and burnt bones in cavern roof. (Photo by Professor Raymond Dart.)

the front, rather than crushed from the side, as would be more likely had falling roof rock dropped on a dead animal. Half a dozen baboon skulls found near Taungs by Camp's party showed this same feature. In all, some forty-two out of a total of fifty-eight crania of *Parapapio* from the three Australopithecine localities were found to have suffered the same drastic injury. Careful study of the fossilized brain cases convinced Dart that not only were the assaults committed while the baboons were still alive, but that in many instances the depressions in the skulls show a double indentation, as if made by the twin condyles on the end of the long limb bone of an ox or some other large-hoofed animal. In other cases the blow seems to have been struck with a wooden bludgeon or a stone weapon. It is impossible to avoid the conclusion that *Australopithecus* chose this way of felling his prey. It even seems possible to deduce that *Australopithecus* was right-handed because, although 64 percent of the damaged skulls from the three localities were fractured by blows delivered directly from the front, 17 percent were struck on the left side as against 5 percent from the right side. The remainder showed attacks from the rear, again usually with the right hand.

No stone implements have as yet been identified. But from the bone breccia at the Makapansgat Limeworks has recently come evidence of a very primitive bone and horn culture recalling similar implements found in association with Peking Man in the North China caves which Breuil regarded as showing definite evidence of human shaping. Taken along with the use of fire attested by the charred bones and signs of carbon, the facts point to the handiwork of an early human being who must have been *Australopithecus* himself.

The Upper Pleistocene age of the two cultural



Brain capacities of Australopithecinae compared with those of other primates. (Courtesy of *Ohio Journal of Science.*)

stages represented in the Cave of Hearths and the Rainbow Cave offers no serious problem, but the date of occupation of the Limeworks Cave—which is clearly of much greater geological antiquity—is not so easily determined. The dolomite formation in which all the caves occur has been exposed at the surface for more than half a dozen million years. Caves could have formed and filled whenever ground-water conditions were sufficiently favorable. Since the various caves appear to have formed at intervals over a long period, their contents should not be expected to be strictly contemporaneous. Indeed, there are sufficient differences between the fossil animal types found along with *Paranthropus* at Kromdraai and those from Sterkfontein, where *Plesianthropus* lived, not a mile away, to suggest that the occupation of those two sites was separated in time by some tens of thousands of years. The precise geological age of the creatures is therefore still a subject of discussion. But the faunal association shows sufficient affinities to those of the Villafranchian horizons in Europe and Southwest Asia to warrant assuming an Upper Pliocene date. This age falls safely within the wider, and somewhat vaguer, limits reached by estimates based on other criteria—physiographic, climatic, stratigraphic, or anatomical. It would place the days of these distant African relatives of modern man distinctly more than a million years ago, and shortly before the onset of the colder climate which heralded the Great Ice Age in Europe and North America.

The site of Troy shows evidence of eleven distinct periods of occupation over an interval of nearly four millennia in early classical times. Some of the prehistoric caves in France have revealed as many as six superposed stages of stone-age culture and have been reinhabited in historical, if not recent, days. The famous Choukoutien locality showed signs of human occupation ranging from that of Peking Man in the early Pleistocene up to Paleolithic and later Stone Age times. It has remained for Makapansgat Valley to reveal near-by cave sites entered by men in the Pliocene Epoch, at two stages in the Pleistocene, and again today.

THIRD HAND

I think astronomers must often feel
Like Alice in a stranger Wonderland,
Watching the telescope's long fingers steal
The stars, and knowing what it means to have
Another hand.

FRANCIS BARRY

GROWING QUARTZ CRYSTALS

E. BUEHLER and A. C. WALKER

Mr. Buehler joined the Bell Telephone Laboratories as an apprentice in 1930 and has been studying quartz crystal growth in the Chemistry Department since 1945. He is now a technical staff associate. Dr. Walker (Ph.D., Yale, 1923) has been a member of the technical staff of the Laboratories since 1923. He has worked in the field of physical chemistry on problems in textiles and fibers, humidity control and measurement, polarography, vitamins, and ammonium compounds. His wartime experience in growing ammonium dihydrogen phosphate crystals for sonar equipment used to detect submarines was employed in the design and operation of a pilot plant for the growing of ethylenediamine tartrate crystals for telephone purposes and in research on the growing of quartz crystals.

THE *Annual Report on the Progress of Chemistry* for 1947 lists practically all the work on the subject of "Crystal Growth" for the previous fifteen years, yet there is no mention of the growing of quartz crystals. The increased activity in the quartz field since 1946 seems to be due to two factors. The first stems from the realization, in 1944, that despite the increase in value of piezoelectric quartz owing to an expanding demand, new producing areas were not being found either in Brazil or elsewhere. The prospect was further clouded by the possibility that export embargoes or other restrictions might be set up to conserve this important mineral resource. The other factor is that Professor Richard Nacken, in Germany, succeeded in growing some large single quartz crystals by a hydrothermal process as the culmination of about twelve years of geological research on the question of the formation of such crystals in nature.[1]

The Bell Telephone Laboratories started an investigation of this subject in March 1946, based on information gleaned from several investigators who visited Germany after the war, particularly Mr. J. R. Townsend, of these Laboratories, and Professor A. C. Swinnerton, of Antioch College. After a relatively few experiments made with equipment similar to that used by Nacken, and with the process he described, it became apparent that Nacken had made substantial progress in the art of growing quartz at temperatures and pressures near the critical state of water, i.e., about 374° C, and 3,200 pounds per square inch.

This report summarizes further progress that has been made in the Laboratories since March 1946. Several methods of growing large single crystals of quartz have been under investigation. One of these has been brought to a state of development recently whereby clear crystals weighing up to nearly one quarter of a pound have been grown in the brief period of twenty days (Fig. 1).

The process by which this crystal was grown is illustrated by Figure 2. Enclosed in a steel tube, or "bomb," having relatively thick walls to withstand the great pressure developed within it, are the three elements needed. These are a "seed" of clear quartz free of inclusions, misoriented areas, and other defects; a layer of broken quartz in the bottom serving as nutrient SiO_2 for seed growth; and sufficient alkaline aqueous solution to fill the free space within the bomb at room temperature to about 80 percent of its capacity. Long before the liquid is heated to the operating temperature indicated—400° C at the bottom of the bomb and 380° C at the top—the liquid expands sufficiently to completely fill the free space, and the pressure then builds up to about that indicated in the figure —15,000 pounds per square inch. The bomb is hermetically sealed at top and bottom with suitably welded steel plugs and is placed on a hot plate. The bomb and hot plate are enclosed in a well-insulated oven, and this simple arrangement is left to "cook" for several weeks. Growth takes place on the seed in an orderly manner according to the following procedure: The broken quartz dissolves rapidly at the very bottom of the bomb in the hot alkaline solution, consisting of 5 percent sodium carbonate, and forms a strong sodium silicate solution, substantially saturated with silica at 400° C. Motion by convection in the liquid is very rapid, and the dissolved silica is quickly transmitted to the slightly cooler region in the vicinity of the seed, where the solution is then supersaturated with respect to silica. This is a basically important condition necessary for crystal growth, and silica deposits out on the seed. Because of continuous motion in the liquid, the growing surfaces are constantly bathed in fresh supersaturated

liquid and the partially depleted liquid is carried down to the bottom, where it is again saturated at the higher temperature. This process is sufficiently rapid to transmit surprisingly large amounts of silica, and the flow across the growing surfaces is uniform enough to produce the clear growth seen in Figure 1.

This relatively simple process was not achieved without considerable effort, expended not only at the Bell Telephone Laboratories, but elsewhere as well.* In order to appreciate the significance of the progress made in this work it may be well to outline some of the important background information, and the necessary steps leading to the success thus far achieved.

The Nacken process. Even in our first experiments using the Nacken process, clear growth was obtained on suitable quartz seeds in weak alkaline solutions. Conditions were somewhat similar to those shown in Figure 2, in that the seed was suspended near the top of the bomb, a supply of nutrient material was placed in the bottom, and the free space within the bomb was partially filled with the alkaline solution. In the Nacken process, however, advantage is taken of a difference of approximately tenfold in the solubility of amorphous and crystalline silica in such alkaline solutions, and the nutrient used was amorphous silica. Furthermore, the bomb was kept at substantially constant temperature at or near the critical point of water, this being 374.2° C. The liquid filling was equivalent to only about 30 percent of the free space at room temperature, and the alkali concentrations were less than about 0.5 percent calculated as sodium carbonate. In addition to the fact that growth takes place owing to the difference in solubility between amorphous and crystalline silica, Nacken attributed significance to a somewhat mysterious mechanism of transport of silica in the vapor phase near the critical temperature of water.

Presumably the process can be carried out at constant temperature because of the large difference in solubility of the two forms of silica, although the results seem to be somewhat better with slight, controlled fluctuations in temperature about the critical temperature, and with liquid fillings somewhat in excess of 30 percent, this amount being just enough to completely fill the bomb with liquid at 374.2°, and is defined as the "critical" volume. In all cases, however, the results were disappointing because, although growth was surprisingly rapid during the first few hours of operation, it was substantially completed in about eight hours and consistently dropped off to nearly zero after the first twenty-four hours. Careful study of the German reports indicated that the German experimenters experienced the same difficulty and had found no satisfactory solution. Nearly 150 experiments were performed in the Bell Laboratories in an effort to control the rate of solution and deposition of silica, using amorphous silica as the nutrient material. Some of the factors investigated were size and shape of bomb;

FIG. 1. Fine example of single quartz crystal in the form of a bipyramid, with unusually clear side, or prism, faces, grown under high-pressure hydrothermal conditions at the Bell Telephone Laboratories in a period of twenty days. The roughness on the top face, accentuated by the lighting used, is superficial and similar to surface irregularities sometimes observed on such faces of natural quartz crystals. The original seed consisted of a piece of irregular Brazilian quartz with no natural faces, but with three roughly formed faces on top approximating the natural angles of the major rhombohedron. The bottom of the seed was a flat face cut normal to the vertical, or Z, axis. The Z-cut face had no surfaces even approximating the proper rhombohedral angles. Growth occurred so perfectly on the top faces as to leave hardly a trace of the "join," and the bottom filled out so rapidly to form the natural rhombohedral faces as to entrap some mother liquor and gas in this cloudy area. All the material below the faintly visible white horizontal line was deposited during the twenty-day period, and this line marks the location of the original Z-cut face.

* Other workers on this problem have made important contributions to a better understanding of the process of hydrothermal growth. Among those with whom we have exchanged information are Brush Development Co., Cleveland, Ohio; Professor A. C. Swinnerton, of Antioch College, Yellow Springs, Ohio; Squier Signal Corp Laboratories, Fort Monmouth, New Jersey; and the Naval Research Laboratory, Washington, D. C.

solution composition and concentration; addition agents; pressure and temperature; degree of fill; agitation, such as rocking, mechanical stirring, simple convection, and cyclic temperature changes; and continuous feeding of amorphous material.

Extremely rapid, clear growth of quartz is possible under certain conditions with the Nacken process, but because of the great difference in solubility of amorphous and crystalline silica it is evident that the process cannot be depended upon to give high growth rates over long periods.

Because growth did not cease entirely after the first day, but continued at a rate of something less than 0.001 inch per day, and also because the amorphous material became covered with a film of redeposited quartz during the run, it was concluded that further experiments were justified, using crystalline quartz as nutrient material.

It follows from equilibrium considerations that if amorphous and crystalline silica are both present as solid phases, the solution in immediate contact with the amorphous phase must be supersaturated with respect to the crystalline phase to the extent of approximately 1,000 percent, due to the tenfold solubility difference. Experience with water-soluble crystals indicates that with more than about 5 percent supersaturation, spontaneous crystallization cannot be prevented. It appears, therefore, that the Nacken growing cycle is somewhat as follows:

Two to four hours are required for the bomb to be brought up to 375° C, and during this time the alkaline solution dissolves silica from the amorphous layer in the bottom at a continually increasing rate, but not fast enough to achieve the 1,000 percent supersaturation with respect to quartz. Sufficient supersaturation develops, however, to promote rapid growth on the seed crystal suspended from the top of the bomb during this period. As the operating temperature is approached, the liquid becomes more and more supersaturated with respect to quartz, and spontaneous seeding occurs, mostly around the individual particles of the amorphous material. Very quickly each particle becomes enclosed in a continuous layer of fine quartz crystals, the local high degree of supersaturation with respect to quartz falls to zero, and growth practically ceases, except perhaps for the very small temperature difference that may exist between bottom and top of the bomb.

There is no need to consider, in the Nacken process at least, a mysterious transfer of silica through the vapor phase, although this may occur.[2] With amounts of liquid present equal to, or in excess of, the critical volume, the seed plate suspended from the top is either completely immersed in the supersaturated liquid or constantly bathed by liquid in the form of strong convection currents or clouds of droplets,† and conditions are favorable for rapid, clear quartz deposition, so long as supersaturation is maintained. Our experience indicated that growths of as much as 0.003 inch in thickness occur during the first four hours, about 0.006 inch in the first six to eight hours, a maximum of 0.008 inch in the first twenty-four hours, and thereafter the rate decreases to the order of 0.001 inch per day or less.

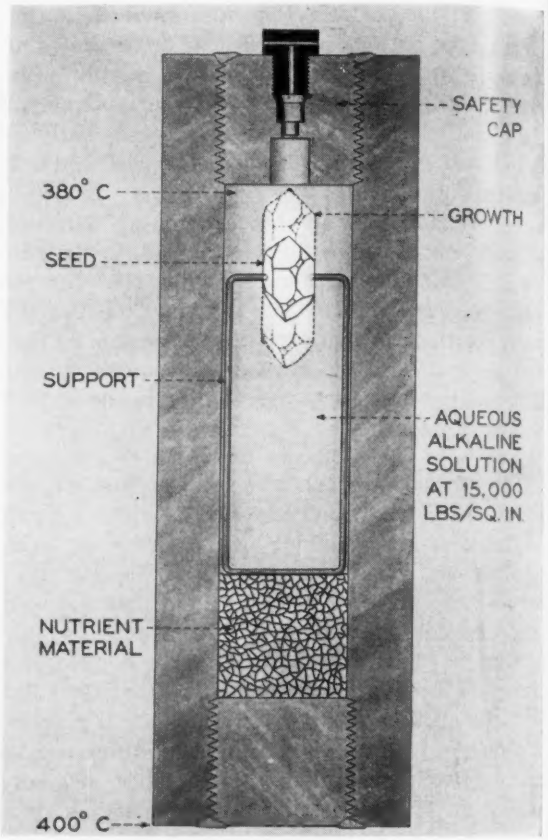


FIG. 2. Section through bomb used to grow quartz crystals.

Growth through density change of solvent. To improve the rate of growth using crystalline quartz

† That violent disturbances occur in the liquid phase within such a bomb at high temperature is indicated by the following: In a bomb 8 inches long by 1 inch inside diameter, the nutrient material—20-mesh quartz—was placed in the bottom in a concentric space around a central wire basket 0.5 inch in outside diameter by 3 inches long, open at the top. After the run at 375° C, with 48 percent liquid filling, *all* the ground quartz was found *inside* the basket which had wire mesh small enough to retain it.

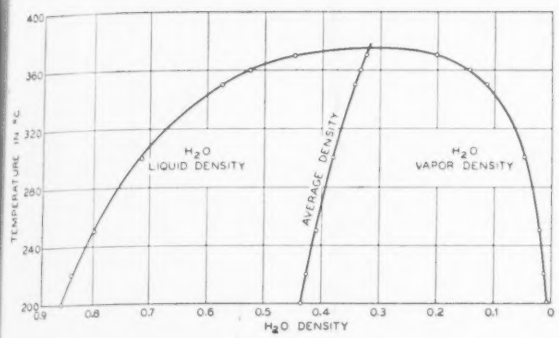


FIG. 3. Liquid-vapor density relations of water near the critical temperature of 374.2° C.

as nutrient material, consideration was given to the possibility of utilizing density changes such as are likely to occur in liquids at the critical point. This may be understood by reference to Figure 3, a graph showing the density changes in water under pressure as the critical temperature of 374.2° C is approached. This temperature is defined as the point at which liquid and vapor have the same density, this being about 0.32 as indicated by the intersection of the average density line with the liquid-vapor density curve. If a bomb is controlled at 372° C ± 2.5° C, the liquid density can vary from about 0.45 at 370° C to 0.32 at 374.2° C, and the vapor density will change from about 0.2 to 0.32 similarly. These are relatively large changes for a temperature difference of but 4° C, and the solubility of silica in either phase should be profoundly affected.[3, 4]

Numerous experiments were done to explore this possibility, and growths were obtained (Fig. 4.) Half of this seed plate was located where vaporization of the liquid occurred near the hot wall of the bomb. Appreciable growth is visible on this half, whereas the remainder is substantially unchanged from its original thickness. These density tests were not encouraging, however, because of the narrowness of the growing zone, and spurious seeds were difficult to avoid. Growth was continuous throughout the run and at a rate comparable with the Nacken process in its early stages.

Higher pressures and temperatures. Three important observations derived from the preliminary work indicated the direction of effort leading to the present successful procedure for growing quartz at high rates. These are:

1. Clear quartz could be deposited at rates as high as 0.03 inch per day, but only for short periods of a few hours, and in the presence of much spurious seeding, using amorphous silica as the nutrient material.
2. Growth at very slow rates, perhaps a maximum of 0.001 inch per day, could be sustained with crystalline quartz as nutrient material, and with no spurious seeds.
3. Growth at a rate of about 0.03 inch per day could be sustained with crystalline quartz as nutrient material in narrow zones within the bomb where marked density changes occurred, and this suggested that better results might be obtained at higher solution density.

To secure such higher solution densities it seemed logical to fill the bomb with more liquid, for at the same operating temperature the greater the percentage fill above that corresponding to the critical volume the greater the pressure. In addition, operating at 400° C, rather than at 375° C, in some preliminary experiments, had seemed to give better results. This direction of attack is consistent with observations made in growing water-soluble crystals at room temperatures, where better results are usually obtained with more soluble salts.

These considerations led to the experiments

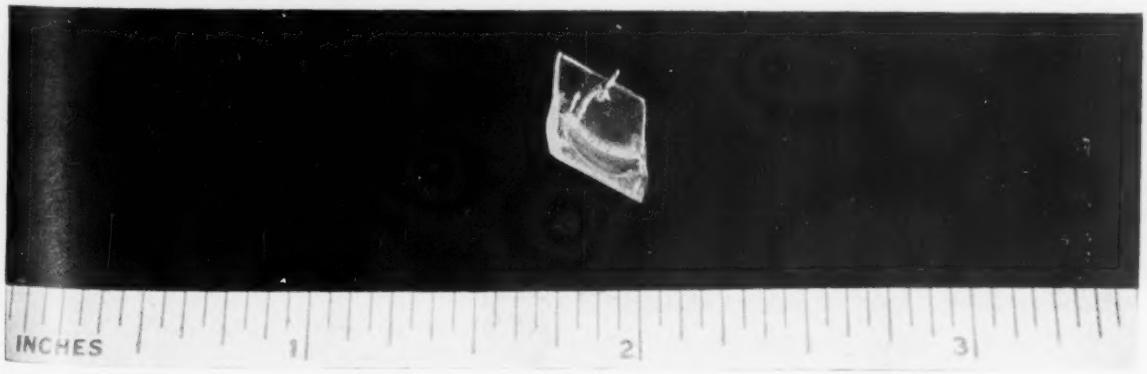


FIG. 4. Quartz plate with growth in zone of density change.

with liquid volumes of 80 percent of the free space at room temperature and with solution concentrations of 5–10 percent sodium carbonate. After overcoming the serious limitations in bomb design and metal creep caused by operating at 400° C, and pressures of 15,000 pounds per square inch, greatly improved rates of growth were repeatedly obtained, up to about 0.1 inch per day. As indicated in Figure 2, the bottom of the bomb was at 400° C, and the gradient to the top was 20° C. These temperatures were measured by thermocouples mounted on the outside wall of the bomb—the internal temperature gradient between top and bottom is probably less than this but cannot be measured with any facilities now available. Tests have been made with bombs having internal diameters of 1 inch, 2 inches, and 3 inches, and essentially the same results may be expected with any size bomb when proper consideration is given to obtaining comparable temperature gradients along the length from top to bottom.

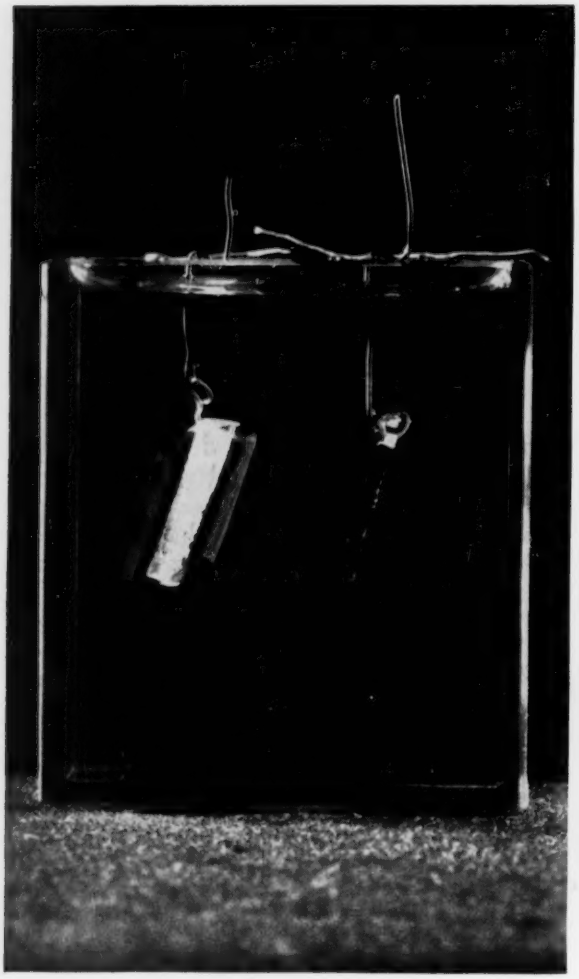


FIG. 5. Quartz crystal on right shows effect of sodium oleate on growth—more growth and less veiling of seed plate.

Nacken reported best results with several alkaline media in this order of excellence: sodium oleate, sodium stearate, sodium carbonate. In our early work, concentrations of these substances in water ranged from about 0.001 percent to 0.5 percent by weight, and in all cases the clarity and smoothness of growth seemed best in the sodium oleate solutions. At concentrations above 0.5 percent, heat decomposition of this sodium oleate seems to seriously interfere with the growth operation. Attention was therefore turned to solutions of sodium carbonate in concentrations up to 10 percent—in some cases with additions of small amounts of sodium oleate below 0.1 percent.

With these conditions, clear crystals weighing up to six grams were grown in fourteen days and with rates as high as 0.028 inch per day. Spurious seeds were completely absent; all the dissolved silica was transported and deposited where desired. Trials made with amorphous silica as nutrient material, for comparison, invariably resulted in a coating of the inner wall of the bomb with spurious seeds, and some such seeds were embedded in the growing crystal.

Figure 5 shows crystals grown with and without the addition of sodium oleate. The crystal on the right was grown in a solution containing 5 percent sodium carbonate and 0.01 percent sodium oleate; the one on the left, without the oleate addition. The seed plates from which these were grown were 3/8 inch square by 1/2 inch thick, cut with the major faces approximately parallel to one of the natural pyramid faces (they are designated in quartz processing terminology as "CT" plates). In general, clear growth takes place only on surfaces cut parallel or nearly so to natural growing faces, and for quartz these are the major and minor rhombohedral faces; therefore the amounts of growth on these two crystals, formed under comparable conditions of time and temperature, may be seen as the relatively transparent amounts on either side of the thin seed plates, shown edgewise in the picture. It is evident that the crystal grew somewhat longer in the presence of the oleate than in its absence, and furthermore the "join" of the grown material with the original seed is much clearer and better in the test employing this addition agent.

More recently we have grown the crystals shown in Figure 6. Seven plates, each 3/8 inch square by 1/2 inch thick, were mounted one below the other

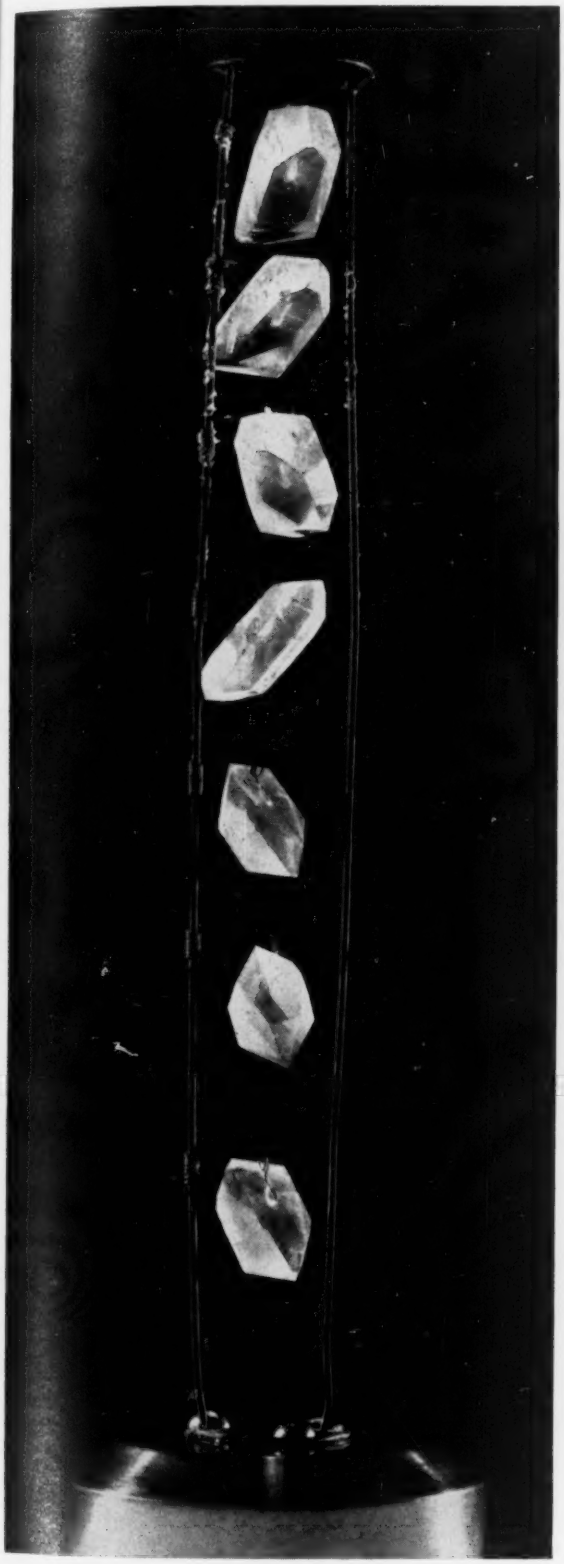


FIG. 6. Arrangement demonstrating growth of several quartz crystals in a single bomb.

in a 1-inch diameter bomb, 12 inches long. The surprising result shown here is that growth in substantial amounts occurs on all these crystals, even on the lower one suspended just above the layer of nutrient material in the bottom of the bomb. Thus it is clear that the entire free space within the bomb above the nutrient material may be used to grow crystals. Furthermore, these crystals grew to lengths of 13/16 inch to 1 1/16 inches in ten days, starting from plate thickness in this case of 0.05 inch. The rate of the largest crystal was of the order of 0.1 inch per day, and that of the slowest, near the bottom, was slightly more than 0.07 inch per day.

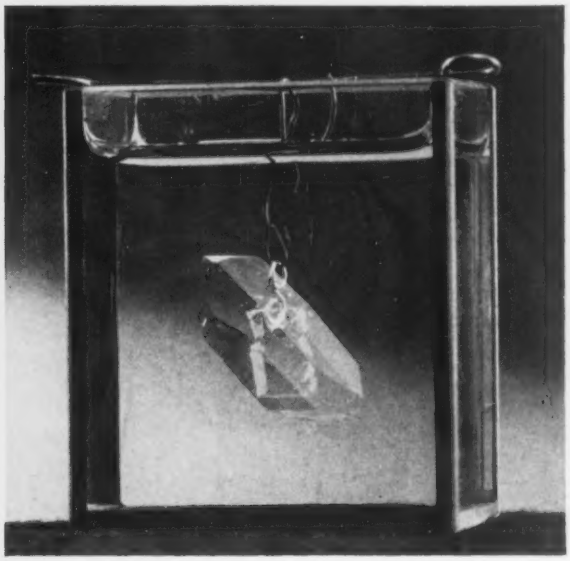


FIG. 7. Quartz crystal in liquid of like index of refraction. Growth is almost transparent, the seed clearly visible.

The growth in this test is clear and unmarred by spurious seeds. This is shown by Figure 7, a photograph of the top crystal in Figure 6, immersed in a liquid having the same index of refraction as quartz, and suitably illuminated. The original seed is plainly visible in outline, but the portions added to each major face are substantially invisible, indicating the clear and perfect quality of growth. The outlines of the full crystal are faintly visible because of slight etching of the surfaces which sometimes occurs during removal from the alkaline liquid in the bomb.

It now appears that the rate of growth of quartz in such bombs, utilizing quartz as nutrient material, is a function of the temperature gradient along the bomb and the size of nutrient material used. Although the true gradient cannot be measured inside

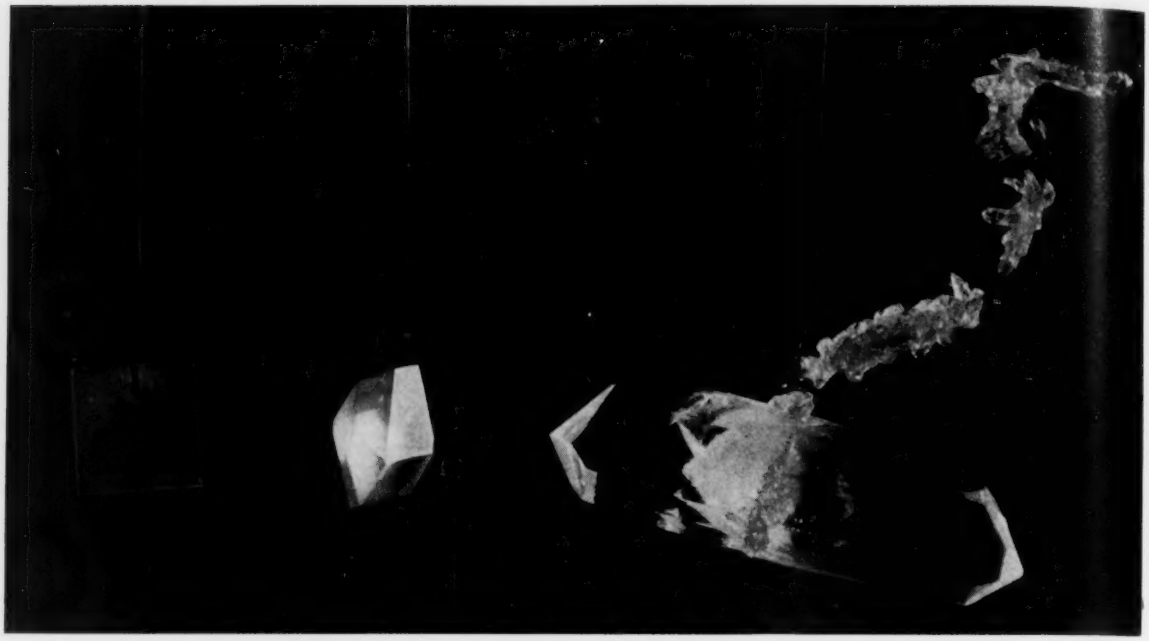


FIG. 8. Effect of bomb leak on quartz growth and spurious seeds.

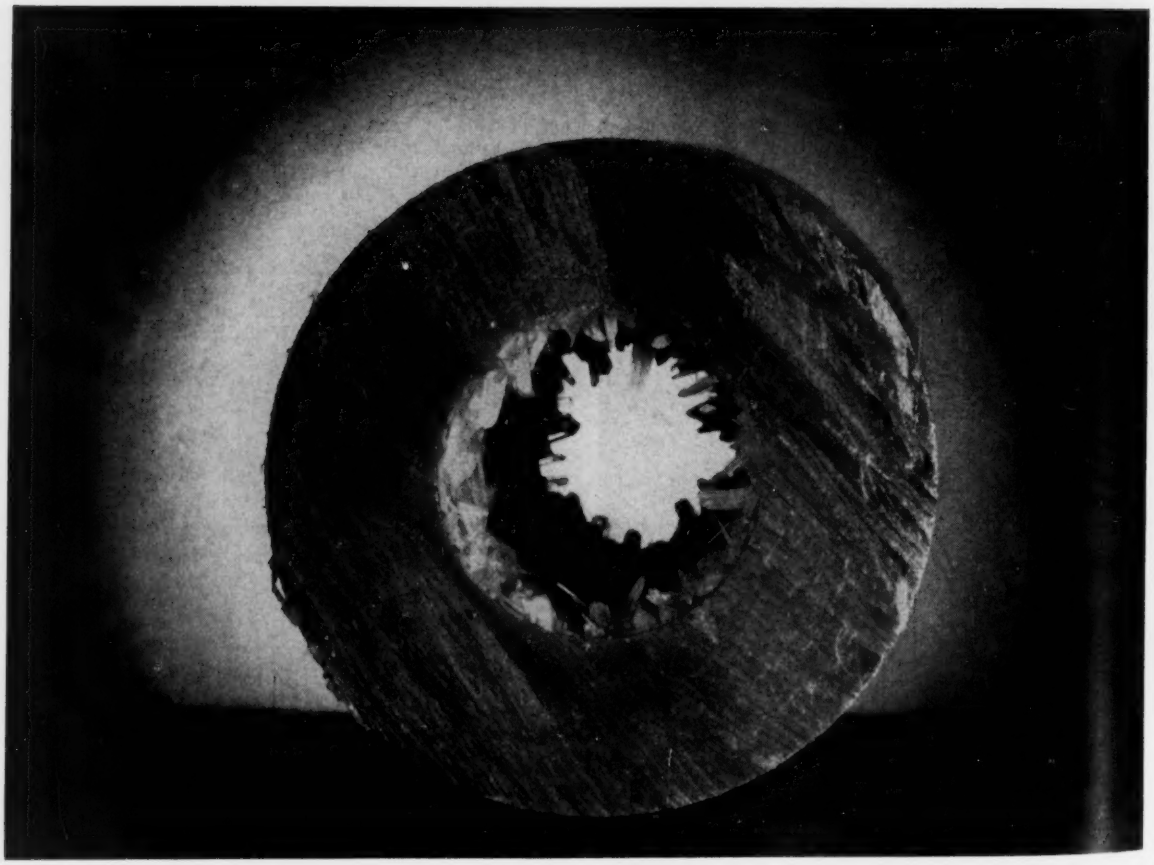


FIG. 9. Section of bomb that leaked, showing geode-like structure of spurious quartz seeds.

the bomb, the apparent gradient as judged by thermocouple measurements made on the outer wall is sufficient criterion for control purposes. It is still too early in the development to quantitatively relate temperature gradient, size of nutrient material, and bomb dimensions to growth rate, but these factors can be correlated, and sufficient progress has been made so that it is clear that large perfect quartz crystals weighing up to one pound or more may be grown at commercially practicable rates under reproducible conditions.

One further development is of interest to geologists. In Figure 8 are shown one large, doubly terminated pyramid, and a smaller, perfectly formed quartz crystal grown from the 3/8 inch square plate shown on the left. The pyramid grew from a double cap seed 1/2 inch long and attained the length of 1 1/2 inches in ten days, or a rate of 0.1 inch per day. This is the first time such a high rate was attained in our work, but it has been repeated many times since. These crystals were grown in two bombs in the same furnace under identical conditions of fill (80 percent), solution (10 percent sodium carbonate), and temperature gradient. The smaller crystals were in one bomb, the bipyramid in another. It was found that the bomb containing the pyramid seed had developed a very slow leak in the top during the run, and about half of the alkaline solution was lost. This leak must have been small indeed, for normal experience with leaks is that all the liquid is lost in a matter of seconds, not days. The rapid growth in the leaky bomb may be explained by one of two possibilities or a combination of both. The leak resulted in a somewhat greater temperature drop in the vicinity of the seed than was the case in the bomb which did not leak, and this caused a greater degree of supersaturation, which increased the growth rate but also gave spurious seeds. Another possibility is that after leaking away part of the fluid the leak sealed itself by silica deposition, and the conditions more nearly compared with those in a bomb having about the critical volume of solution.‡ This smaller amount of liquid may have resulted in a two-phase condition inducing much more violent convection currents than would be the case in the single-phase system. Strong density changes between liquid and vapor may aid greatly in promoting rapid growth. No spurious seeds were found in the bomb which did not leak, and the crystals grew in this bomb at about 0.02 inch per day.

The conditions inside the bomb which leaked, as shown in Figure 9, resembled to a noteworthy degree the conditions reported as being present in some natural quartz deposits, called geodes. Perhaps natural quartz crystals did not require thousands of years to grow, as has been postulated. The escape, through cracks in the earth's surface, of superheated alkaline solution containing dissolved silica may have resulted in the formation of the large natural quartz crystals in a matter of days or months rather than centuries.

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1. Office of Technical Services Reports, PB 6498, PB 18,784, PB 28,897. Dept. of Commerce. Washington, D. C.
2. Ingerson and Morey, *Econ. Geol.*, 1940, **35**, 772–85, found large deposits of quartz as fine needles near the top of a steel bomb in which many experiments had been performed at high temperatures and pressures, and they concluded that the silica must have been transported to this point through gaseous water.
3. Hannay and Hogarth, *Proc. Roy. Soc., London*, 1879, **29**, 324, described the deposition of a solid from a liquid on suddenly heating the liquid being held just below the critical temperature so that it is vaporized in a pressure tube, and attributed this deposition of the solid to rarefaction—change in density of the solvent.
4. Kennedy, G. C., *Econ. Geol.*, 1944, **39**, 25, described the changes in solubility of silica glass in water on passing through the critical temperature region, and concluded that the data "strongly suggests that solubility in the gas phase is not only a function of temperature, but is very much an inverse function of the specific volume of the water vapor."

‡ An 80 percent fill, with 10 percent sodium carbonate solution, at 400° C, may not be much above the critical conditions for this concentration. Therefore, it is possible that the bomb without a leak contained but a single liquid phase under these conditions, whereas the leaky bomb may have had two phases near the end of the run.

SOME OF MY BEST FRIENDS ARE SCIENTISTS

F. BARROWS COLTON

Mr. Colton is president of the National Association of Science Writers and a member of the editorial staff of the National Geographic Magazine. *He was for four years on the staff of the Springfield* Republican *and for five years a reporter and science writer for the the Associated Press. He was the sole correspondent at sea with the U. S. Atlantic Fleet on war maneuvers the year of Pearl Harbor and in 1947 was a member of the National Geographic Society–U. S. Air Force solar eclipse expedition to Brazil.*

JUST over one hundred years ago, when Dr. W. T. G. Morton demonstrated ether anesthesia in Boston, it was a newspaperman, Albert Tenney, of the Boston *Evening Journal,* who publicized the story and helped bring the new technique to the attention of surgeons. There is nothing new, therefore, in the interest of the press in science as a source of news, nor in its ability to render a real service to science through publicity.

The use of ether anesthesia to relieve the pain of surgery was big news in 1846, which was why Albert Tenney wrote the story. Tenney was no science writer, in today's professional sense, but he was an alert newspaperman who knew a good news story when he saw one. That is still the most important fact about science writers of the present day. They are primarily reporters looking for news stories that will interest their readers. In other respects, science writing has come a long way since Tenney's day. It has become a valuable adjunct of science, an important aid to public education, and a rapidly growing specialty in the field of journalism and radio.

It is probably safe to say that science writers understand scientists better than scientists understand science writers. But science writers are so important to science that scientists need to have some knowledge of their working methods and problems.

In the process of writing about science, modern science writers not only are covering the news but are rendering a real service to science as well. Publicity is gradually giving the public not only more knowledge of science but an understanding of, and respect for, the scientific method of working and thinking. It is helping to build up an interest in science among the people who pay taxes and contribute to endowments to support research.

No longer does popular fancy place the scientist in his old role of a kindly but rather peculiar old chap with a beard who spends his time in such seemingly obscure pursuits as chasing butterflies or collecting rocks. The scientist has become the man who released atomic energy, developed radar and television, made rain with dry ice, and performed near miracles with new medical and surgical techniques. His prototype is no longer the shy professor but a clean-cut, rather youngish fellow, alert and up to date, who is doing things the layman realizes to be practical and worth while.

This increase of popular interest in, and respect for, science has resulted in a growing use of science news by newspapers, magazines, and radio, and a corresponding rise in the number of science writers, who report and explain the news of science to the layman. They perform an essential and specialized role. They are in a very literal sense the "interpreters" of science, translating its variegated technical dialects into words that the newspaper, magazine, and radio audience can understand. That audience, incidentally, includes many scientists who cannot possibly keep up with all developments outside their own fields.

From small beginnings, science writing has grown into an important, well-established, responsible branch of journalism. Science Service, endowed in 1921 by the newspaper publisher E. W. Scripps, pioneered in the serious reporting of science for newspapers. Once this agency was almost alone in the field, but today there are scores of science writers on the staffs of the press associations, individual newspapers, and weekly and monthly magazines, which is the best possible testimony to the soundness of Mr. Scripps' judgment in encouraging popular science writing. In the magazine field, a pioneer was my own publication, the *National Geographic Magazine,* whose editor, Dr. Gilbert Grosvenor, began publishing accurate, understandable science articles as early as the turn of the century. These were profusely illustrated, for photographs, especially in color, often tell a story better than words. Today all magazines appealing to the general reader cover science.

When I began writing about science nearly twenty years ago, full-time science writers were

few, and they had to work under the handicap of the bad name that many newspapers had acquired by treating scientific meetings as subjects for the staff humorist, by inaccuracies, and by oversensationalism.

Today the National Association of Science Writers, organized in 1934 to promote accurate, responsible science reporting, has nearly 100 active and associate members, with high standards for admission to membership. There are many able interpreters of science in the information and public-relations offices of government agencies, universities, research foundations, and industries. A number of these are associate members of the N.A.S.W. Many other writers who do not cover science full time report it ably on occasion.

The N.A.S.W. is an affiliated society of the American Association for the Advancement of Science. In 1938, only four years after it came into existence, the organization was awarded the Clement Cleveland Medal for its contributions to the control of cancer through publicity. To encourage new science writers, the AAAS administers the George Westinghouse Science Writing Awards, through which two prizes are given annually by the Westinghouse Educational Foundation for the best newspaper and magazine articles on science. Thus today science writing has become a well-recognized, respected, and generally accepted adjunct of science in the United States. It is helping to gain support for science and to educate laymen in scientific thinking.

When I want to plague some of my scientific friends, I tell them how I became a science writer. One day in the Associated Press office in New York my boss, never noted for wasting words, sent for me and rather bluntly asked: "Ever write any science?" I admitted having covered a few assignments on scientific subjects. "Ever have any science in school?" I confessed that I had had two bouts with chemistry in high and preparatory school, in both of which I came off the loser, but that in college I had passed in botany and geology. The boss's expression did not change at this hardly impressive recital of scientific background. He said: "How would you like to go down to Washington and cover science?" I said I would like to try it.

Contrary to what some scientists may darkly suspect, this story does not prove that the newspapers or magazines think so little of science that they carelessly assign any unqualified, uninformed reporter to cover it. Its real point is that a science writer first of all must be a reporter. He must have the layman's point of view. In writing his material he must think and write like a layman, not like a scientist. He knows what interests the layman and, equally important, how much the layman can understand and absorb. His training qualifies him to do this. The scientist's training usually does not.

Of course any science writer must learn about science, and he does, partly by reading, but even more through his daily experience in covering science news. Science writers try hard to build up their background, to get the news straight and write it accurately, but they cannot do the job alone. It is up to the scientist to learn how to explain his work so that the writer understands. With a little thought and effort this can be done.

Once the press service director of the AAAS telephoned me at the Associated Press to say that some abstracts of papers to be read at a forthcoming meeting of the American Mathematical Society in Minneapolis had been sent to him. My first reaction was that there could not be any news in mathematics. But with the patient help of a mathematician we were able to pick out a few subjects that would have some popular interest when the implications of the results were explained. Half a dozen news stories were written which described the applications of some of the mathematicians' work but contained no mathematics whatever.

Newspapers printed the stories. They helped show the public that mathematics does have some practical use. One newspaper editor congratulated the AP on having a reporter who understood higher mathematics. (I've never heard the last of that one!) Thus the science writer can successfully cover a subject of which he knows little by enlisting the aid of scientists who can explain it and because he himself knows what will interest the reader.

Before a small group of scientists and writers discussing science and the press, I once made the rash suggestion that it would be very helpful if all scientists had at least a brief course in journalism, or something akin to that, as part of their training. Of course this aroused anything but enthusiasm. If scientists did have such training, however, they would acquire a better understanding of the problems of the science writer, and would be better able to explain and write about their own work in clear and interesting fashion. It is significant that the winner of the 1948 AAAS—George Westinghouse magazine science writing award was a scientist who had once worked on the Brooklyn *Eagle!*

To illustrate the pitfalls of scientific jargon for the layman, Dr. George R. Harrison, of MIT, tells this classic story as reported in the Washington *Star:*

A New York plumber of foreign extraction with

a limited command of English wrote the National Bureau of Standards and said he found that hydrochloric acid quickly opened drainage pipes when they got clogged and asked if it was a good thing to use.

A Bureau scientist replied:

"The efficacy of hydrochloric acid is indisputable, but the corrosive residue is incompatible with metallic permanence."

The plumber wrote back thanking the Bureau for telling him the method was all right. The scientist was a little disturbed and showed the correspondence to his boss—another scientist. The latter wrote the plumber:

"We cannot assume responsibility for the production of toxic and noxious residue with hydrochloric acid and suggest you use an alternative procedure."

The plumber wrote back that he agreed with the Bureau—hydrochloric acid works fine. A top scientist—boss of the first two—broke the impasse by tearing himself loose from technical terminology and writing this letter:

"Don't use hydrochloric acid. It eats hell out of the pipes!"

Any science writer will tell you that one of his perennial problems is getting scientists to explain their work in simple, brief, clear language. When he finds one who can, he is likely to be suffused with a happy but unfamiliar glow. Any magazine editor can tell you sad tales of his struggles to get eminent scientific contributors to write interesting and understandable articles.

All this is clear enough, since most scientists have no training in popular writing, are accustomed to thinking and speaking in technical terms, and have little conception of how limited a background the average layman has in science. There are exceptions—some scientists are outstanding in their ability to speak and write about their work in simple terms. Albert Einstein has done a good job of explaining his own theories of relativity. Dr. Alexander Graham Bell once said that a good scientist could explain his work understandably to any intelligent layman.

On the other hand, the science writer is an expert, in his way, in the task of explaining science to the layman. He does it, sometimes, in ways that some scientists do not like, but in general that is because they do not understand his problems. One problem is to write a story that people will read. If it is not read, it might as well not be written. The scientist reads technical journals as a duty to keep abreast of developments, but the newspaper or magazine reader feels no such compulsion. If a newspaper or magazine science story does not arouse his interest, he will quickly turn to something that does.

Also, to get a science story printed at all, the writer must arouse the interest of a busy editor who is pretty ruthless about what does or does not get onto a crowded press association wire or newspaper page. He must therefore put the main interest and point of his story in the first paragraph, and do it in words that not only summarize the story but tell it interestingly enough so that the reader will read on.

If someone has discovered a successful treatment for the common cold, that is what the reader wants to know. He has little interest in the history of the search for such a treatment, unless the story is unusually dramatic and colorful. A science news report must be written with the essential facts stated first, followed by the name of the person or persons who did the work, and what little background is necessary for understanding what was done. This is exactly the opposite of the procedure in writing a scientific paper, in which the previous work upon which the present result is based is meticulously outlined, with proper credit to all concerned, and with the new material placed near the conclusion.

Space, too, is one of the science writer's problems. On the jam-packed wires of the Associated or United Press it is a rare science story that rates more than 400 words. Even individual newspapers seldom give their staff science writers much more space than that, unless the story is a truly big one. Actually, 400 words or so is about all the average newspaper reader wants to bother to read. Into that brief space the writer usually must crowd enough essential details to make his story accurate, enough background to show why it is important, enough color to make it readable, and the names and affiliations of the scientists who did the work.

Time also is forever prodding the reporter. Newspaper and magazine deadlines are inflexible. An Associated Press reporter, for example, knows that somewhere in the United States some newspaper that takes his service is going to press almost every minute of the entire day or night. Time especially is a factor when a science writer is covering a scientific meeting. His reports of the addresses on the program usually must be written before the papers are delivered if they are to get to the newspapers on time. Accounts of papers read at a morning session, for example, normally appear in the afternoon newspapers of the same day. An afternoon newspaper is made up very early in the morning, so all but the most important news stories that appear in it must be written the night before.

A morning newspaper is made up during the previous evening and night hours; therefore, news reports of papers read at an evening session likewise must be on the way to the newspapers about the time the speaker is being introduced, and preferably earlier. If a reporter is forced to go and listen to an evening address, his story must be written so late that it probably will not get into the next morning's paper at all.

Press association reporters have even earlier deadlines. Their stories for morning papers must be moving out on the wires by 4:00 or 5:00 P.M. the day before, and for evening papers by the previous midnight. All this has been said many times before, and many scientists have paid attention and have been cooperative, but the point bears repeating again.

It is of vital importance that speakers at scientific meetings provide complete copies of their papers *in advance* to the press, if possible at least twenty-four hours ahead of the time of delivery. Then the reporter can write his story at some leisure, with all the facts before him, and he has time to seek out the speaker for additional particulars if necessary. If he has no copy of the paper, he must try to waylay the speaker whenever and wherever he can to learn what he is going to say, bothering him perhaps at inconvenient times, and making hasty notes that are far less satisfactory than a copy of the paper, and more likely to result in an inaccurate or incomplete story.

Even an abstract is better than nothing, provided it really summarizes the speaker's conclusions. But if you want to make a science writer see red, wave before him an abstract such as this: "A new treatment for the common cold will be discussed, with results of its application to 1,000 volunteer subjects. The percentage of favorable responses will be stated, and the chemical composition of the drug and method of administration of the treatment outlined."

In the case of unusually technical papers, it is extremely helpful to provide a brief nontechnical explanation of the main points in addition to a copy of the paper itself. The extra time this takes probably will be no more than the speaker otherwise would have to spend in explaining the more obscure points of his paper to reporters who would seek him out for enlightenment anyway. He will make a real contribution to the good relations of science and the press and to the public's better understanding of science.

Many scientific organizations already are doing a good job of making available complete information not only about the papers delivered at their meetings but also concerning their interim activities.

Another thing to be remembered is that all reporters want to get what is known as "a good play" for their stories. This means that they try to produce news reports that will be given front-page space in a newspaper, or at least a top-column position on an inside page, or a correspondingly prominent position in a magazine. For a press association reporter, a good play means that a high percentage of the newspapers receiving the service use his article prominently. Obtaining a consistently good play means prestige and salary increases for a reporter. It is the best possible stimulus to his morale.

Most important factor in getting a good play is the subject matter of the story, which must have the widest possible interest for all types of readers. That is why medicine gets a better play than phytopathology, and why making it rain with dry ice gets more headlines than the discovery of a fossil dinosaur in Wyoming.

No honest science writer will distort facts to get a good play, but he will use all legitimate means to make his story interesting. He is likely also to keep the headline writers in mind, and to insert in his lead paragraph, if he can conscientiously do so, a word or phrase that will fit conveniently into a headline and attract attention to the story. After all, he is striving to get people to read his report.

Here is a good place to emphasize again that reporters do not write the headlines on their stories. This is done by the newspaper copy desk, which coldly appraises the news value of every story that goes into the paper, and strives to write a headline that will summarize what a story is about and attract attention to it. (Critics of headlines might try writing a few sometime—they would soon find that only a certain number of letters and spaces will fit in a line between the two sides of a column.)

Another much-discussed point in science writing is what constitutes accuracy. No science writer worthy of the name is ever deliberately inaccurate, and takes all possible care to avoid it. Yet, inevitably, someone is accused of being inaccurate, of using wrong emphasis, of telling only part of the story, or of failing to give proper credit to all who have contributed to some scientific accomplishment.

One veteran science writer has truly said that there are two kinds of accuracy, equally legitimate —that of the technical journal and that of the newspaper (or magazine) report. No one is going to take the popular account and use it as a guide to reproduce the work that it describes. Any scientist who wants further information will get in touch with the person who did the work. Edward

E. Slosson, first editor of Science Service, once said: ". . . The would-be popularizer is always confronted by the dilemma of comprehensible inaccuracy or incomprehensible accuracy and the fun of his work lies mainly in the solution of that problem."

Ninety-five percent, probably, of the laymen who read a science story will be interested only in the details given there. Anything more would be boring if not incomprehensible to them. Further, they will remember only the gist of the story, very likely not much more than was mentioned in the first paragraph or two. Any attempt to give full technical details would make the story unreadable and dull. A news story can be accurate in essential details without being complete. As another science writer has said, a science news story need be accurate only to the first decimal place. Such a story is far more likely to be printed and read, and to give science favorable publicity, than a long, technical article so filled with meticulous detail that it is over the heads of most laymen.

For the same reason, it is unwise to burden a story with a long list of names and affiliations of all those who have contributed even in a minor way to the result. These names mostly will mean nothing to newspaper readers. They will take up precious space and bog down the story's readability.

In some ways the magazine science writer has an easier problem than a newspaperman. His space is more generous, he can cover a subject more fully, and more often can take time to have his copy carefully checked. Otherwise, however, the magazine science writer must follow the same general rules as the newspaperman. From the first sentence his articles must try to catch and hold the attention of the reader. The reader of any popular magazine wants to be entertained, or at least informed in an entertaining and interesting manner. He cannot be expected to read an article that does not hold his interest. So the magazine science writer must strive constantly to hold his reader's interest, to be crystal clear, to avoid dullness at all costs, to inject humor, thrills, anecdotes, and human interest. Though he can use more detail and background than the newspaper writer, he must make these as interesting, dramatic, and colorful as the main point of the story.

Science writers sometimes feel that some scientists are oversensitive about publicity, somewhat lacking in a sense of humor and proportion. No one wants scientists to adopt the classical politician's attitude of "I don't care what you say about me as long as you mention my name." But the cause of good publicity for science would be helped if scientists would venture a little further from their ivory towers, if they would develop slightly thicker skins at times, and not eternally condemn all the press merely because in one isolated instance they have had their work written up in what they consider a somewhat inaccurate or incomplete way. One reason for this sensitivity, of course, is the reaction of the scientist's own colleagues, who may criticize him severely, or at least kid him about the news story almost as though he had written it himself. Science writers find it hard to understand why a man should be blamed for getting publicity when he has done something of legitimate news value.

Publicity for science pays off in unexpected ways. News stories about research in physics and the sense of smell at Yale brought that university large financial gifts for further work along those lines. A news story about stone dart points made by Folsom man in New Mexico brought a flood of letters to the Smithsonian Institution showing that such points had been found all over the United States, indicating a far wider distribution of this ancient people than had been realized. A scientist published in a technical journal an account of raising bigger and better baby chicks by feeding them tobacco of high nicotine content as protection against disease. It attracted little notice, but a newspaper story about it brought him a flood of inquiries from chick raisers.

Good science writing is not easy. In his technical journals the scientist knows he is writing for an audience that understands his specialized language. The science writer must try to reach an audience that ranges all the way from scientists themselves to people with extremely limited background. I still remember the telegraph operator, sending an Associated Press dispatch, who looked up from the copy and said, "What's this word 'physics'? I never heard that before."

A few science writers have acquired so much knowledge of science that they seem to have lost the layman's approach. They get too technical. I read one article by such a science writer that was so obscure in its meaning that I had to get a physicist to explain it to me.

In trying to set forth the science writer's point of view, I may have seemed to preach at the scientists. This is done, however, in a friendly spirit. Mutual understanding is the best way to promote good relations between scientists and the press. Some of my best friends are scientists!

NEW WORLDS FOR STUDY

JAMES HILLIER

Dr. Hillier (Ph.D., Toronto, 1941) has been a research engineer at the RCA Laboratories, Princeton, New Jersey, since 1940. He has specialized in electron optics, microscopy, diffraction, and microanalysis. His article is from an address given during the AAAS Centennial Celebration in Washington, D. C., September 1948.

IN AN absolute sense one might consider including the electron microscope in a series of lectures concerning giant machines for research to be an error, for it is indeed a dwarf compared to the new particle accelerators. On the other hand, what it lacks in physical size it gains by the breadth of its applications.

In the scientific study of the structure of matter, man's interest has shifted to progressively smaller "fundamental" particles. The work with the large accelerators concerns the study of the present-day fundamental particles and represents the frontier of our basic knowledge in science. By contrast, the electron microscope is used to study a range of larger structures which, for various reasons, including lack of instrumentation, has received scant attention in the past. This is the range in which the molecules are organized into the solid structures with which we deal in everyday life. Although the work with the electron microscope cannot be considered fundamental in the present physical sense, it can be considered as representing the frontiers in many sciences. Now the biologist, metallurgist, geologist, industrial chemist, engineer, or, for the matter, anyone interested in the organization of molecules into heterogeneous solid structures can begin to link his studies with those of the physicist and chemist. More specifically, the electron microscope provides the only means for studying the heterogeneous organization of solids between the level of the large molecule and that which is an order of magnitude above the resolving limit of the light microscope. It is obvious that the number of structures to be studied in this range is very large and that the electron microscope has, in fact, provided science with a new world for study.

The magnitude of this new world is difficult to comprehend and even more difficult to describe. When we discuss the capabilities of the electron microscope, we customarily use the term "useful magnification." At the present time we say that the best electron microscope can provide a useful magnification of 200,000 diameters on a suitable specimen. This is a large number, a number that falls in the same category as many numbers of interest to the astronomer, and quite beyond true comprehension by the human mind. In an attempt to realize some of the significance of this number, let us consider the role of the electron microscope in another way. Let us suppose, as an example, that a research worker took as a project the examination under the electron microscope of an entire plant or an entire animal. He would soon find that he would not live long enough to complete his work. A very rough calculation would show him that it would require forty years to take just a fast glance at the structures in one square inch of the surface of his object. If he wanted to photograph that square inch for careful study at the highest magnification, he would find that more than six thousand years would be required for the job. When we consider that all the objects of nature involve organization in volume, the time required to make the complete study of even a small portion of that object again goes beyond the powers of comprehension.

A scientist has no other means of depicting the heterogeneous organization of matter in the range of dimensions extending from the limit of the light microscope down to the level of a large molecule. In the study of crystals the very regular arrangement of the molecules makes them susceptible to study by interference techniques, whereas in the case of simple plant viruses and of some animal viruses it is the uniformity and simplicity of external shape that make them susceptible to studies with the ultracentrifuge, with polarized light, etc. Since these constitute only an extremely small proportion of all the structures that may be encountered in this range, it may be said that the new world opened by the electron microscope is a completely unexplored one. The only landmarks in it are those around its edges. They are to be found in the studies made near the limit of the light microscope or in the studies made by chemists interested in the larger molecules. Quite obviously, the scientific explorer who ventures into this new world must use these landmarks as bases for his expedi-

Electron micrograph of the initial stage of germination of a spore of a soil bacterium (*B. mycoides*). Obtained in collaboration with Dr. George Knaysi, Cornell University. (Magnification, 21,000×.)

tion and must maintain close contact with them throughout his research; otherwise he is in great danger of becoming hopelessly lost.

At the present time a survey of the literature showing the results obtained with the electron microscope would reveal, in a rather striking way, that the valuable work is being done by those groups, and only those groups, who have a thorough understanding of this situation. At the same time, such a survey would reveal that, as yet, no group has succeeded in penetrating very far into this new world. We have on the one hand a number of workers who use the techniques of the high-molecular-weight chemists and physical chemists as a base of operation. These groups use light scattering, low-angle X-ray scattering, X-ray diffraction, the ultracentrifuge, electrophoretic measurements, polarized light measurements, etc. as the starting point for their explorations with the electron microscope. On the other hand, a number of workers, in particular the bacteriologists, the metallurgists, and the industrial chemists, have used the high-powered light microscope as their starting point. It is by no means my intention to belittle the already important contributions of the electron microscope, but it is nevertheless my opinion that the really important contributions of this new instrument will come when these two groups of explorers begin to make contact in the inner regions of this new world.

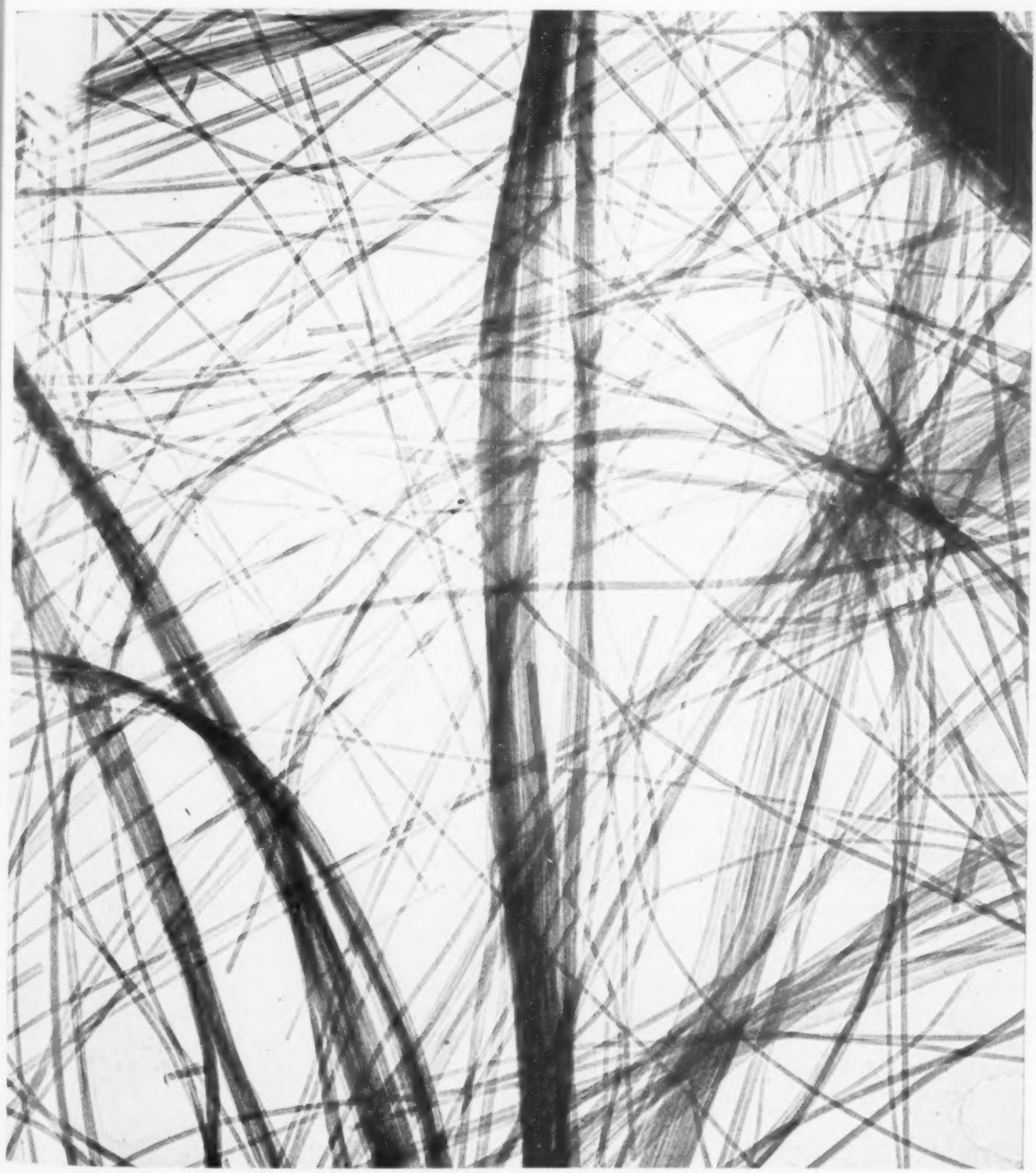
I

Let us turn now to a more specific examination of the electron microscope in an effort to define more precisely its place in scientific research. As most readers are undoubtedly aware, the resolving power of the light microscope is limited by the relatively long wave length of the illumination used. This is a fundamental limitation which prevents any microscope from resolving object points separated by less than half the wave length of the illumination used. In other words, a light microscope using visible light can never be expected to produce useful magnifications in excess of approximately 1,000 diameters. If ultraviolet is used, this limiting figure may be raised to 2,000 or 2,500. The electron microscope owes its existence, at least in part, to this limitation and to the fact that almost twenty-five years ago L. deBroglie suggested that electrons must also have something periodic in their nature. In other words, a wave length could be associated with them, and, according to DeBroglie's formula, that wave length would be very small. For electrons of the velocities used in the electron microscope that wave length is roughly 1/100,000th that of visible light. DeBroglie's work did not immediately bring up the possibility of an electron microscope with high resolving power. That concept had to await publications of H. Busch, in which he showed that any electric or magnetic field possessing axial symmetry would act as a true electron lens for the electrons passing very near to the axis of symmetry. Shortly after Busch's work it was pointed out that there was in effect a complete analogy between geometrical light optics and geometrical electron optics. Thus, in principle at least, it would be possible to construct electronic counterparts of any optical equipment. In the early 1930s a few research workers were already interested in constructing electron microscopes, and over the succeeding years the development has been very rapid, with all the scientifically active countries contributing their share to that development. There are now several highly practical instruments produced commercially in various parts of the world. During its development the fundamental design of the electron microscope has changed very little. In fact, that development has involved mainly the study and understanding of the most fundamental form of the instrument.

As has been stated in the literature many times, the electron microscope is a rather exact analogy of a light microscope. Thus, the light beam is replaced by an electron beam, and the sequence of glass lenses—the condenser, the objective, and the

eye lens—is replaced by a corresponding series of electric or magnetic field lenses. The electronic system differs physically in that the path of electron beam must be highly evacuated, so that tricky mechanical devices are necessary to permit insertion of the specimen and the image-recording means in the microscope. It differs also in that the electronic image is always projected as in photomicrographic apparatus and that it is necessarily observed visually only after transformation into a visible image by means of a fluorescent screen. This is done during the operation of the instrument, but a photographically recorded image is always made for subsequent careful study.

In use in research problems the electron microscope provides an image, in a true geometrical



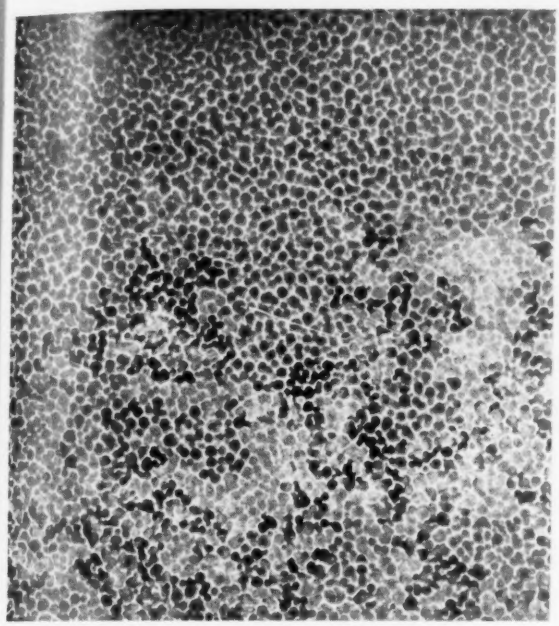
Asbestos fibers. At this magnification the asbestos fibers take on the appearance of fine tubes. (30,000×.)

sense, of the object placed in it. This image differs considerably in quality from the image produced in a light microscope. This is in addition to the fact that the image possesses better resolution. When the electron microscope was first introduced, the most commonly voiced objection to it was that it would be exceedingly difficult to interpret the images obtained. Such objections were justified only if their authors realized that exactly the same objections could be raised in the use of a light microscope. It is only in an exceedingly small fraction of the use of any microscope that interpretation is made by a direct analysis of a single image. Instead, it is much more common practice to use the microscope merely as an aid to our sense of vision, in which method the individual or isolated image becomes valueless. It is only from the relationship of one image to many others, from the experience of the microscopist, and from the conditions of the experiments that the value and interpretation of the microscope image are obtained.

As with the light microscope, the images produced by the electron microscope often possess ambiguities that can be resolved only on the basis of extra experiments. Since these ambiguities are quite often the result of the specific mechanism of image formation, it is not surprising that those encountered in electron micrographs are quite different from those encountered in photomicrographs. To understand them, it is necessary to go further into the mechanism of image formation in the electron microscope.

The electron microscope is essentially a monochromatic instrument, and hence differentiation of the image points must be produced by variations in intensity. In the electron microscope, these differences in intensity are produced for the most part by differences in the electronic scattering power of the different object points. The electrons irradiating an ideal object point constitute an extremely sharp, solid cone, at the apex of which the object point is situated. Some of the electrons that traverse the specimen are scattered, however, so that the electrons leaving the object point constitute a rather diffuse and somewhat broader cone than the incident electrons. It should be pointed out, in passing, that absorption of electrons by the specimen does not play a significant part in the determination of the intensity of the image points. The electrons in the outer edges of this cone are prevented from reaching the corresponding image point, either by an apertured diaphragm in the objective lens, or by other characteristics of that lens. This causes diminution of the intensity of the image point. The electronic scattering which occurs within a given point depends upon the thickness and the density of the specimen at that point, and hence the variations of intensity in the image are, in a sense, a measure of the variations in the mass per unit area of the specimen. This leads to the first ambiguity that is encountered in electron microscope images—namely, that in a single image it is not possible to determine whether a reduced intensity corresponds to an increase in thickness or to an increase in density. In practice, there are numerous methods that resolve the above ambiguity. These methods usually involve demonstrating the three-dimensional geometry of the specimen independently of the density differences. Stereoscopy, which can be accomplished at high magnifications, or shadow-casting are two possible methods.

An ambiguity that is much more difficult to resolve arises with regard to the dependence of resolving power on contrast. In light microscopy, it is well known that the numerical aperture of the objective determines the resolving power and also has some (though little) effect on the contrast obtainable in the specimen. In the actual use of the light microscope, however, contrast is usually considered to be independent of the resolving power and is introduced by means of specific absorption such as that produced by specific staining. In electron microscopy, as already indicated, contrast is obtained by differentiating among the differing scattering powers of the object points. This control is accomplished for the most part by the aperture of the objective lens, but this aperture also controls the resolving power by controlling the aberrations in the uncorrected objective and by controlling the diffraction effects. Experimental work has shown that those apertures which exercise a significant control over the contrast lie in the range where the diffraction effect is of appreciable magnitude. Thus resolving power and contrast in the electron microscope are very closely related and competing phenomena. Moreover, in organic specimens in particular, the contrast produced by chemical differences in the structures of the specimen are usually so slight as to be unobservable in the image. Thus, the nonexistence of differentiation in the electronic image of many biological structures does not necessarily indicate a lack of structural differentiation in the specimen. The same problem has of course existed throughout the use of the light microscope, but for the most part it has been adequately solved by the use of selective staining techniques. At present only the most elementary and primitive selective staining



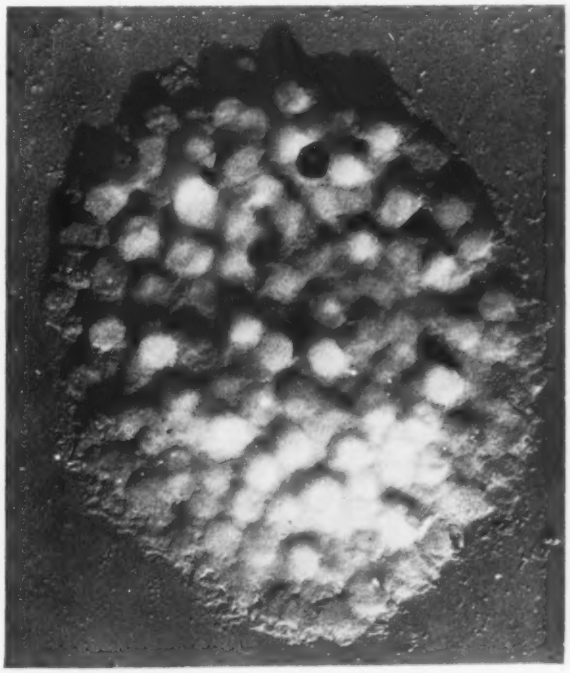
High-magnification electron micrograph of a partly crystallized evaporated film of antimony. (45,000×.)

methods have been tried with the electron microscope, though the possibility of their effective use definitely exists.

The third ambiguity that should be pointed out is for the most part due to the present method of operation of electron microscopes. The control of contrast by an appropriately sized aperture leads one into the range of aperture sizes, which are difficult to make and maintain and in general tend to limit the resolving power of the instrument. In the common use of the electron microscope the aperture sizes are considerably larger than those which exercise a significant control over the contrast. Contrast in that case is obtained by the differential action of the lens aberrations on the widely scattered rays and on localized phase-contrast phenomena.

The exact details of this effect are too complex to discuss here, but the results are of sufficient importance to be described. One of these is the production of a variation in the contrast and the resolving power from point to point of a single image. This means that in certain parts of an image the resolution and the contrast will be the maximum obtainable with the system, whereas in other parts of the *same* image the resolution and the contrast will have deteriorated to the point where they will not be much better than could be obtained with a light microscope.

It is not unusual for a light microscopist to attempt to push his interpretations beyond the resolution shown in the images. Since in most cases he has no alternative, there is a certain justification in his method; however, he can expect to receive the criticisms and objections of his contemporaries. In electron microscopy, on the other hand, the situation at present is more serious. The somewhat inexperienced electron microscopist can fall easy prey to the error of making interpretations in some parts of his image where the resolution may be as bad as 1,000–1,500 Angstrom units on the basis of a resolution of 50 Angstrom units demonstrated elsewhere in the *same* image. This error frequently finds its way into the permanent literature and is not picked up by the worker's potential critics, because very few of them are as yet completely aware of these possible difficulties of interpretation. No better example of this is to be found than in the literature concerning the use of the electron microscope in the field of bacteriology. As experience has been gained by the electron microscopists, and methods of preparing bacteria for the electron microscope have been improved, it has become a more or less accepted fact that the healthy vegetative bacteria cell is completely devoid of internal structure. These developments have convinced most bacteriologists that the structures seen by the early electron microscopists were artifacts of preparation. We are now convinced that this lack of differentiation in our ideally pre-



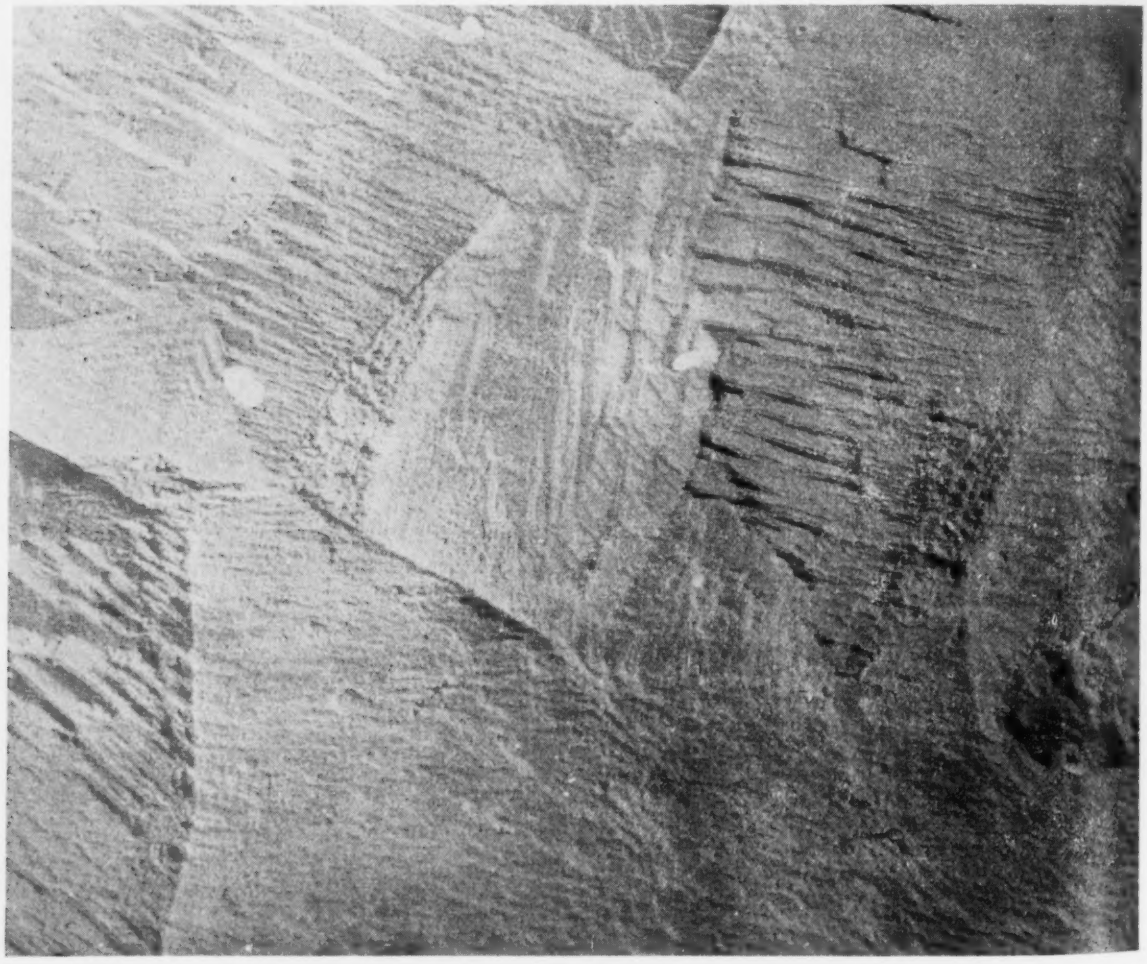
Chloroplast from a spinach leaf. Specimen provided by Dr. J. Turkevich, Princeton University. (Magnification, 10,000×.)

pared specimens is due entirely to lack of contrast in the electron microscope images. It is not to be inferred that we consider this an undesirable situation to be in. On the contrary, we are now in an excellent position to start experimenting with techniques designed to introduce contrast in specific ways and thus delineate in a controlled fashion the structure of the bacteria.

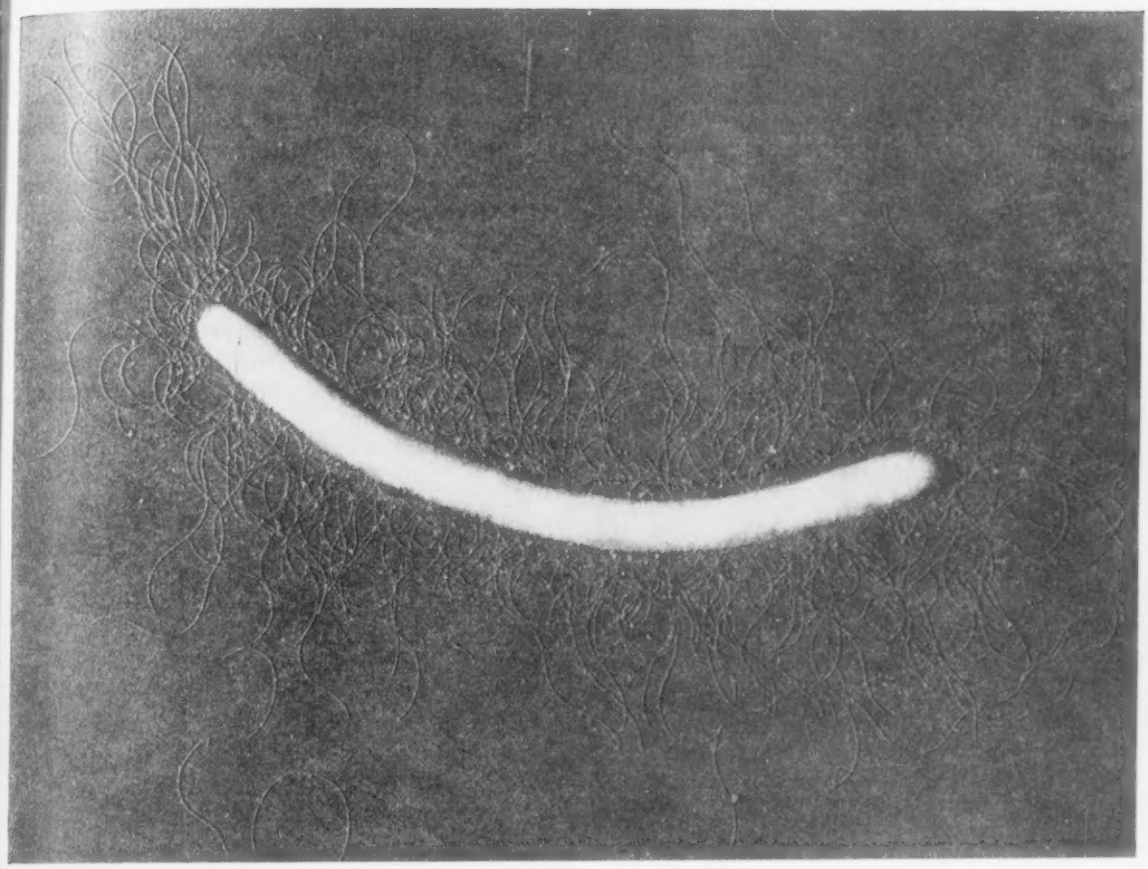
II

At this point we should discuss another aspect of electron microscopy. The specimens we prepare for examination on the electron microscope must satisfy a few rather stringent conditions. The conditions themselves do not present any particular problem as far as providing a specimen of which it is possible to get a good electron microscope image. There is in addition to the technical requirements, however, a more fundamental requirement that the final prepared specimen must provide either an accurate representation of the specimen as it was in its original state, or at least if distortions have occurred that they be *known* distortions.

The technical conditions are simply that the specimen must be sufficiently thin, dry, and also that it must be capable of withstanding a vacuum and a certain amount of electron bombardment. It it now known that the last two are invariably satisfied with the first two. These conditions introduce no particular problems for inorganic specimens. In biology, however, the problem of obtaining a specimen which is at once thin and dry and representative of the original specimen is indeed a difficult one. The actual difficulty lies not so much in satisfying these conditions simultaneously as it does in the fact that there are no independent criteria for deciding when they have been fulfilled. This means that both the investigation of structures and the determination of their validity must



Deeply etched surface of a rolled copper sheet. (21,000×.)



Swarming proteus, showing a large number of flagella. (Specimen prepared by Dr. C. F. Robinow.) (7,300×.)

be carried on with results obtained from one instrument. The method is straightforward enough but does require considerable work and some imagination on the part of the microscopist. It involves preparation of the specimen by as wide as possible a range of techniques and a careful study of the results produced by each, with a view to discovering those structures that are persistent in the techniques or to devising a structure that could act as a point of origin for all the variations observed. The ultimate desire is, of course, the development of a technique of specimen preparation which provides an accurate representation of the specimen and thus requires no multiple check.

The electron microscopist now has at his disposal a range of reliable techniques for every conceivable type of solid material. These include satisfactory sectioning techniques for tissues, growing techniques for the study of bacteria, replica techniques for the study of almost any type of surface, and dispersion techniques for the study of any type of particles or fibers. The accuracy of representation obtained in the electron microscope images has been satisfactorily checked in many cases, with the particular exception of the thin tissue sections. The technique for cutting such sections is so new that there has been, as yet, no opportunity to investigate the different fixing and embedding techniques or to determine the accuracy of the representation except insofar as it has been possible to compare results with those obtained with the light microscope. In these cases, at least, the correlation has been excellent.

As was indicated in our initial analogy, the number of problems to which the electron microscope can be applied is almost beyond comprehension. This is particularly true in the biological sciences, where a complete understanding of any individual entity requires knowledge of the heterogeneities of the structure to the point where the physical chemist can take over. This amounts to almost a two hundredfold improvement in resolving power over that of the light microscope. But resolving power is defined on a linear scale, whereas all real structures are, of course, three-dimensional. In effect, this means that to give a complete description of biological entity requires obtaining 10^6 as much information as can be obtained at the limit of a

light microscope. This figure is somewhat frightening because it gives a clear-cut illustration of how little we know of these structures. At the same time, it should give encouragement to the graduate student looking for research problems. It should also provide him with a warning to choose from the many problems an example that is at least representative of a large general group of structures. It is only by such a judicious selection that he can hope to do research of lasting value. In my laboratory, for instance, two of us have made a total of 40,000 exposures in the past nine years. Taking as a rather conservative mean value for the magnification, 10,000×, we now realize that in this time we have photographed *only one square millimeter of our world.*

III

In the preceding, I have discussed the more general role of the electron microscope in scientific research. This discussion should not be closed without mentioning a rather specific use of the electron microscope which, although trivial compared to the broader considerations of the above, nevertheless constitutes at present a major part of its application in industry and research. I refer to the use of the instrument in the measurement of the particle size of pigments, catalysts, viruses, etc. It then becomes merely a means of expanding the scale of a ruler. Even in this application the electron microscope bears little resemblance to most scientific tools. For instance, to obtain a mean value for the particle size of a given material, it is necessary to compile the statistics from measurements made on individual particles. If the material has a very uniform particle size and shape, the results obtained with the electron microscope seldom do more than confirm those provided by indirect measurements. On the other hand, the electron microscope can provide results when the sample is heterogeneous with regard to particle size and shape, a case in which the indirect methods break down.

An attempt has been made in this article to describe the electron microscope primarily as an extension of our sense of vision, providing access to the world of heterogeneous structures represented by the first and succeeding levels of organization of molecules into solid structures. This world is a new one in that no other means for studying it has been available. It is an extensive world into which the scientific worker can proceed only with caution and with considerable deliberation as to the general value of his work.

THE CARIBBEAN ISLAND ECONOMY

CARLOS E. CHARDON

Dr. Chardon (Hon. Sc.D., Dartmouth, 1936) is director of the Instituto Agrícola Nacional of the Dominican Republic. He was called to the post from Puerto Rico, where he was Commissioner of Agriculture, Chancellor of the University of Puerto Rico, Executive Director of the Land Authority, and Director of the Institute of Tropical Agriculture. He has conducted agricultural surveys for the governments of Colombia, the Dominican Republic, and Venezuela, and has recently been appointed general manager of a modern sugar mill being erected near Cuidad Trujillo.

THE economic problems of the Caribbean islands cannot be approached in their overall aspects—with reference to planning, operation, and actual solution of some of the present dilemmas—unless students of the area take into consideration the close geographical relationship between the islands. We must consider first that the Caribbean is a *geographical region*; and, second, that we are dealing also with a series of *political entities*. For more than three centuries the political patterns have prevailed over the geographic reality, which has brought about a disarticulated economy among islands forming part of an archipelago. In considering separately the political units and their problems, by far the majority of students have neglected the fact that in a sound economy the political units (without interference with their sovereignties or those of the metropolitan governments) may in many cases play an important economic role with reference to the economy of the others. So far, efforts have been isolated and fragmentary; the perspective of the whole has been ignored, and each country, or each colony or group of colonies, has fought its own battle, trying to solve its problems independently.[1] This reminds us of the man who failed to see the forest for the trees.

The above statements are self-evident: we know of half a dozen plans for the economic reconstruction of some of the political units of the area; the files of the Colonial Office in London are full of elaborate reports of West Indian Royal Commissions. Dozens, perhaps, of able and well-balanced brains and scores of pounds of stationery have been used with little or no practical result. To make it more forcible, until recently there has never been an economic concept which represents an integration of common interests and which would serve the mutual needs of the geographic group.

Recent reports, however, indicate that the problem of the economic integration of these islands is occupying the minds of a selected few. I myself expressed it very emphatically in my 1937 report to President Trujillo.[1] Erik Williams[2] recently wrote that trade in the Caribbean follows the flag and that each unit is geared toward some export market in which it enjoys tariff protection in return for certain obligations. Thus each unit forms part of some economic bloc. Further on, he says that interinsular relations are virtually nonexistent and that the Caribbean is, in fact, a geographical expression. It is really a collection of individual units, each functioning independently of the others. Colonel Standley, Secretary of State for the British Colonies, advocated "some organization which will enable them to find some solution of problems which are as common to Porto Rico or indeed to Cuba and Haiti as to Jamaica. If the Caribbean is to survive and prosper it must be fitted to the world as a whole."

I

A number of American scholars have pointed out that there has been an integrated Caribbean policy, at least on the part of the United States. Chester Lloyd Jones, for example,[3] has shown that even before the middle of the nineteenth century, a Caribbean policy began to develop in the United States (p. 6). Further on, after touching on the subject of the results of the Spanish-American War and the building of the Panama Canal, he says:

> Broader than the interest in Cuba or in the Canal, has been the Caribbean policy of the United States. The political phases of this policy have developed from the inability of the weaker states to maintain normal conditions within their own borders and in their relations to other nations. Its economic features . . . touch the problem of the safety of the United States.

The problem is, even admitting that there has been such a policy, that there has been no over-all planning. When Robert T. Hill, of the United States Geological Survey, presented for the first time to American readers a vivid description of

the West Indies,[4] he discussed rather thoroughly the economic and social problems of each island, but unfortunately missed the picture as a whole. This eminent geologist failed to recognize the Caribbean as a functional geographical area in the economic sense. This attitude of studying *pêle-mêle* is typical of the imperialistic ideas of his time. As a result of the war with Spain, the American Mediterranean had just been opened up for American capitalistic enterprise, and Hill candidly admitted that he belonged to a nation of "Yankee traders." "This compliment," he says, "although not so intended, classifies us among the mostly highly civilized nations, which are those which excel in commerce, and signalizes our need of foreign markets."

When this statement was made Hill perhaps did not realize that he was not speaking for the United States alone; the same practices had already been followed for three centuries by "good old Spain" and the "highly civilized nations" of Great Britain, France, and the Netherlands. Even pacific Denmark once had a small bite of the highly nutritious and well-seasoned West Indian pie. We must admit, however, that colonial policies have changed greatly since World War II. The problems facing the territories and colonial possessions of the sovereign nations are so complicated that a Caribbean Commission, which includes representatives of the United States, the United Kingdom, France, and the Netherlands, is now actively engaged in studying these problems and proposing practical solutions to them. It appears that in past years outside interests ate too much of the pie and left too little for the natives. The problem now, as we see it, is to restore an equitable portion of the whole menu to the natives and persuade the barons to be satisfied with a smaller share. In plain words, the work of the Caribbean Commission is to prescribe a reducing diet to the barons and by all means possible to improve that of the natives.

C. L. Jones, in commenting on the condition of the European colonies of the Caribbean, makes the following statement:

> All the island colonies, with the partial exception of the Bahamas, have in our day become black man's islands in which the population has increased to the point where the locally produced food supply is insufficient to meet the demand.
>
> Most of them [the colonial islands of Europe] have not had an economic development sufficient to allow collection of public revenues greater than are necessary for the maintenance of the minimum standards of order, communications and public health.

The Caribbean Commission is defective in that its members are dealing with less than one third of the geographical area of the islands. Thus the commission covers a limited portion of the area, and its efforts, even if successful, will suffer that limitation. With the results already achieved and the experience acquired by the Caribbean Commission, perhaps the establishment of a much larger commission comprising representatives from all the area may be advisable, but such a proposal naturally will have to be dealt with in the higher circles of diplomacy.

Most of the islands of the Caribbean area, because of their single-crop system of agriculture, are obliged to import large amounts of foodstuffs from distant countries, for they cannot themselves produce them in competition with a highly protected product such as sugar. To be sure, the economy of by far the vast majority of the islands is based traditionally on one export product: sugar. They are known as "sugar islands" because in many of them—from Cuba, the largest, to tiny Barbados—sugar is king. Sugar accounts for a large share of the cultivated acreage—four fifths in Antigua, three fifths in Cuba, two fifths in Puerto Rico. The sugar industry is the greatest single employer of labor in the Caribbean and dominates its external trade to the extent of nine tenths in Barbados, four fifths in Cuba, and nearly two thirds in Puerto Rico. It accounts for nearly nine tenths of the freight hauled by the public railway systems of Cuba and Puerto Rico.[2]

This situation is accentuated by the fact that the sugar industry is dependent upon the outside world not only for its market, but for its capital ownership, and control. Foreign ownership is implied in the term "absenteeism," and the physical remoteness of the owners adds to the distance between social classes which springs from race or color or nationality or culture. The proprietor tends to be outside of, not part of, the local or national community.[5]

With reference to these dangers, three economists who made careful studies in Puerto Rico have expressed the following judgment:

> It is often considered an evil for a country to be largely dependent economically upon the production of one commodity. Such a situation, usually increasing exposure to the vicissitudes of the so-called business cycle, is held to constitute a potential source of economic instability; for, with a country's eggs all in one basket, the danger of sudden dislocation and loss, both cyclical and non-cyclical, is likely to be greater than were its economic activities diversified. This evil is likely to be aggravated when the product in question is an export commodity, dependent upon external markets, and consequently one whose price is subject to the influences of a world market and world prices.[6]

Be this as it may, the sugar economy is so profoundly entrenched in the economic framework

of these islands that we do not foresee any substantial deviation from it in the future. Moreover, a rapid change might upset the present economy—and why should there be any change if with few exceptions the only steady policy of the capitalistic mother countries involves sugar?

During World War II, however, the dangers of the single-crop system were clearly demonstrated. Because of the scarcity of shipping facilities, there was a sudden decline in the imports of foodstuffs. At the close of the great conflict, the postwar economy, through tradition and necessity, had to be cast in prewar molds, and the much-talked-of diversification of crops was again put aside. (We cannot dismiss this subject without saying that during the peak of the submarine campaign, in 1943, the Anglo-American Commission (now the Caribbean Commission) was established. This newly created agency dedicated its efforts primarily to the supplying of foodstuffs for the territories and colonial possessions of the United States and the United Kingdom. A system of convoys was adopted and the situation was greatly relieved.)

II

In the midst of a great scarcity of food in the "sugar islands" the Dominican Republic mobilized a part of its resources as a food-producing nation and was able to supply a substantial part of the needs of Puerto Rico and other neighboring islands. The fact that this country had escaped the influence of sugar capitalism, and had dedicated a major portion of its efforts to the development of a diversified system of agriculture, suddenly placed it in a unique position as a food-producing nation in the Caribbean.

This performance points out potentialities for the future, precisely because within the Caribbean area the Dominican Republic is the *exception* to the rule. The country does not need to go into a multiple system of crop production; indeed, it already has a diversified agriculture—more so than any of the neighboring islands. For this reason, it is a country of far more food-export potentialalities than any other area in the Caribbean region.

Before going into the export business, the country had first to do everything possible toward cutting down the imports of the main articles of food. Statistics show that the imports of such staples as rice, lard, vegetable oils, butter, cheese, prepared meats, and soup pastes (*pastas alimenticias*), which, during the decade 1920–29, amounted to $36,059,400, dropped to $10,008,500 in 1930–39, and to $447,171 during 1941–45.

Along with this significant reduction came an appreciable increase in the export of foodstuffs to the other islands of the Caribbean. From $3,440,124 in 1942 this figure rose to $9,335,932 in 1946. The other islands benefited through these food exports were, in the order of importance: Puerto Rico ($5,970,637); Dutch West Indies ($1,746,533); Cuba ($962,950); French West Indies ($550,766). The balance was distributed unevenly between the British West Indies and the Virgin Islands.

The great purchasing power of Puerto Rico is derived mainly from (*a*) the sale of agricultural products, plus the sale of the products of its increasing industry; and (*b*) from the considerable sums of money contributed by the Federal government, especially for the armed forces. Under the first item, the exports during the fiscal year 1946–47 were sugar, molasses, and rum, $95,567,000, or 53.5 percent of the total; tobacco, $9,125,000, or 9.1 percent; and fruits (fresh, canned, or preserved), $3,562,000, or 2.0 percent. This makes a total of 64.6 percent. In industrial products, needlework stands out with $39,268,000, or 22.0 percent, of the whole. For the Dominican Republic, the exports during the calendar year 1947 were as follows: sugar and molasses, $54,260,306, or 65.2 percent; cacao beans, $12,951,938, or 15.6 percent; coffee, raw or roasted, $5,126,412, or 6.2 percent; and tobacco, $4,561,435, or 5.5 percent. Thus the total of classical export products of Puerto Rico amounted in 1946–47 to $108,254,000, whereas those of the Dominican Republic during the calendar year 1947 amounted to $76,900,000.*

But now appears a very important difference between the economy of both countries. The Dominican Republic, with a diversified agriculture, imports relatively small amounts of food and food products. In the calendar year, the figure was only $2,704,681; in Puerto Rico, during the fiscal year 1946–47, the figure was enormous, amounting to $95,481,494. Furthermore, the imports into the Dominican Republic of $2,704,681 are balanced by exports (mostly to other islands of the Caribbean) during the same year of $3,167,271 in edible animal and vegetable products. Thus the country is practically self-sufficient from the standpoint of food. No other country or colony in the whole Caribbean area has such a record.

There is still another very important item that should not be forgotten. During the fiscal year

* It is to be regretted that the official statistics of both countries are not strictly comparable, since those of Puerto Rico are based, following the usual procedure in the United States, on fiscal years ending June 30, whereas those of the Dominican Republic are based on calendar years ending December 31.

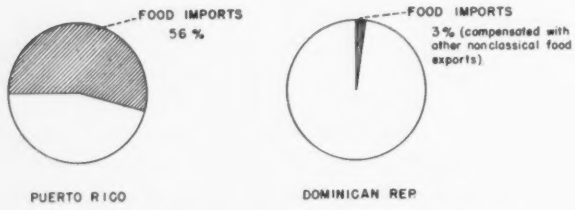


FIG. 1. Some comparative statistics.

1946–47, Puerto Rico imported $10,539,512 of unmanufactured wood, sawmill products, and wood manufactures. The Dominican Republic, during 1947, imported only $476,720, a figure that is more than balanced by an export of forest products amounting to $1,044,842.

Figures 1 and 2 show that for every dollar obtained from products exported from Puerto Rico, $0.56 has to be spent in purchasing food and food products abroad. In the Dominican Republic, for every dollar derived from export products, only $0.03 is spent on imported food, but this is more than balanced by the sale of nonclassical exports of food to the other countries of the area.

III

Self-sufficiency as an economic goal is not necessarily an index of a sound economic organization. It is generally looked upon by modern economists as a utopia. Among a group of islands like those of the Caribbean, however, the case must be viewed under a different light. If these islands have been for centuries exporters of classical food products such as sugar, tobacco, coffee, and cacao to the mother countries, and they have to depend on considerable imports of other foods to subsist, the problem of food supply may at any time be magnified by another emergency such as the last world war. The Dominican Republic is satisfied with its present economy, based mainly on crop diversification. The economics of the other islands are too tied up with the historical and economic policies of the past to expect a change in them; furthermore, such change may in many cases not be desirable. But the fact that, within the Caribbean area, there is a country that has deviated from the paths of the others, sharply differentiates it from its neighbors. If adequately developed, with some help from the Food and Agricultural Organization of the United Nations, the Dominican Republic may play an ever-increasing role as food producer and supply, at least in part, the needs of some of the other islands.

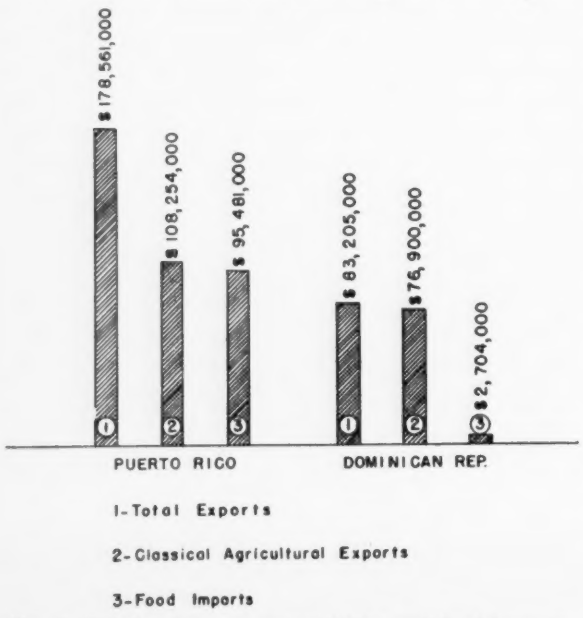


FIG. 2. Relation between export dollar and food imports.

What we have said about Puerto Rico in relation to the Dominican Republic applies to several other Caribbean islands where a sugar economy prevails. Underdeveloped countries are generally underfed countries. It is against the rules of sound economic collaboration to have underfed populations hampered by tariff barriers which preclude the purchase of food from near-by places that produce animal and vegetable products at relatively low costs. The Caribbean region, however, continues to be—with little hope of change—a political mosaic on the maps. There are seven nationalities represented within the archipelago. It is a political and economic rainbow which contrasts with the uniform blue ocean that surrounds it. We have paid dearly in the past for the lack of economic conscience among the economic leaders of the various countries and colonies which comprise the group. And the Caribbean Commission, as it is at present constituted, is destined to be by the very force of its limitations what may perhaps be called an "unfinished symphony" in the economics of the Caribbean.

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HIGH-SPEED AIRCRAFT AND AERODYNAMIC HEATING

VICTOR HALBMILLION

Victor Halbmillion was educated in Paris, France, studying theoretical engineering at the Ecole Centrale and law at Paris University. He then graduated in mechanical engineering from Columbia University, with a degree of Master of Science. Mr. Halbmillion is senior research associate in aerothermodynamics research at the Naval Ordnance Laboratory, White Oak, Maryland, and relaxes from his work in heat transfer and aerodynamics by indulging his love for winter sports, animal photography, and table tennis.

WHEN any object travels in the atmosphere at high speeds, its temperature increases owing to a phenomenon called aerodynamic heating. Until the last few years, this phenomenon was overlooked, mainly because at the speeds formerly reached by conventional aircraft it was still unimportant. With a steady increase in the speed of passenger-carrying airplanes —which have now invaded the supersonic range— and with the recent developments in the field of guided missiles—where speeds as high as several times the speed of sound are a commonplace occurrence—the problem of aerodynamic heating becomes of major importance and may prove to be the primary limiting factor to the speed of flight in the atmosphere.

The mechanism of aerodynamic heating is a combination of several complex effects. If we assume a sharp-nosed body moving in the atmosphere, the air in front of the body will be compressed and will suffer a considerable temperature rise. The temperature reached is called "stagnation" temperature, because relative to the body the air in that region remains practically still. The stagnation temperature can be fairly easily calculated by applying the classical formulae of gas dynamics, but the phenomena which take place along the side of the body are much more complicated.

If we consider a volume element of air at a very large distance from the body, it is obviously almost undisturbed by the passage of the body and has an absolute velocity zero (assuming no wind or other disturbance). On the other hand, a similar volume element in the immediate proximity of the body "sticks" to the body and is carried by it along its flight. Relative to the body, it will appear to have a velocity zero, and, as a first approximation, conditions apparently similar to stagnation are realized.

The air temperature in the immediate proximity of the skin is not, however, equal to the stagnation temperature, because of thermal effects in the boundary layer (which is simply the total volume of air around the body, at a given instant, which has been disturbed by the passage of the body). For practical purposes a volume element of air is considered disturbed if the motion of the body has impressed upon it a velocity in the direction of motion equal to at least 1 percent of the flight velocity of the body. Across the boundary layer the velocity of a volume element of air increases as a function of its distance to the wall of the body. The volume elements of air sliding in the boundary layer at different velocities produce shear stresses, and the work performed by these stresses increases the amount of heat produced. On the other hand, a certain amount of heat dissipation results from the fact that the temperature changes across the boundary layer, the peak temperature being reached in the immediate proximity of the body. Since there is always a heat flow between a point at a higher temperature and a neighboring point at a lower temperature, some of the heat produced in the inner layers flows across, and is finally transferred outside, the boundary layer.

The actual temperature resulting from the effects described above is called "boundary-layer temperature" (which is an abbreviation for the expression "boundary-layer temperature in the immediate proximity of the body") or "impressed-plate temperature." The relationship between the stagnation temperature and the boundary-layer temperature is mainly a function of a flow parameter called the Prandtl number, which is equal to the product of the specific heat of the fluid by its absolute viscosity divided by its thermal conductivity. When the Prandtl number is smaller than one, the amount of heat dissipated across the boundary layer is larger than the amount of excess

The Curtiss XF-87 in flight.

heat produced by the shearing stresses, the conditions being reversed for a Prandtl number larger than unity.

The Prandtl number depends to some extent on the temperature of the atmosphere, but mainly on its composition. For air, it is always smaller than unity, which is a favorable condition, and the boundary-layer temperature rise is usually only approximately 90 percent of the stagnation temperature rise (above the ambient temperature). A further favorable factor is that at extremely high stagnation temperatures, a certain amount of dissociation of the oxygen and nitrogen molecules takes place, which reduces to some extent the boundary-layer temperatures, but this is actually a very minor consideration, for the temperature-reducing effect of dissociation occurs only when the boundary layer is already very much above the practical allowable limits, as will be seen.

The boundary-layer temperature is a first approximation of what the temperature of the outer skin of the body would be should this temperature reach an equilibrium. The actual body temperature would be somewhat less, because some of the heat received by the body is reradiated to the outside (a relatively minor effect, however). Also, the body does not assume equilibrium conditions immediately, but passes through a transient period, the duration of which depends upon the heat-transfer coefficient between boundary layer and body skin, which is the amount of heat transferred per unit time and unit area of the body for each degree of temperature difference between boundary-layer and body temperature. This coefficient, usually denoted by the symbol h, increases with the speed of the body, but decreases when the density of the atmosphere, through which the flight takes place, decreases. The equilibrium temperature is therefore reached fastest during high-speed, low-level flight. At altitudes of the order of 100,000 feet and above, the reduction in air density is such that even at very high flight speeds the heat transfer into the body remains relatively very small.

In the discussion that follows, the equilibrium

conditions will be assumed to have been reached, and the heat transfer by radiation neglected. The body temperature rise is then approximately given by the boundary-layer temperature rise, which varies as the square of the speed of flight, and is shown in Figures 1, 2, and 3. The significance of such graphs is easy to interpret. Let us first assume a commercial-type, passenger-carrying airplane flying at altitudes between sea level and 35,000 feet. We shall assume the temperature of the atmosphere to vary according to the NACA tables for the Standard Atmosphere, the results of which are presented in Table 1.

By assuming that a passenger cabin temperature of 70° F. is required for comfort, the maximum boundary-layer temperature rise allowable, and therefore the maximum flight velocity attainable without artificial refrigeration of the cabin, may be calculated (Table 2).

It can be seen that the problem of aerodynamic heating is still of little concern to the designers of commercial planes. High-speed commercial flights will take place at relatively high altitudes, 20,000 feet and above, in order to take advantage of the reduced drag in a rarefied atmosphere, which would allow speeds as high as the speed of sound without any provision for refrigerating equipment. The same problem is already of importance to the designers of military fighters and interceptors, and planes equipped for attempting speed records. Speed-record flights are usually made at very low altitudes (only a few hundred feet) because of regulations set up by the Federation Aeronautique Internationale. They are attempted on hot days and in hot locations because, all other conditions being equal, the drag is lowest when the atmospheric temperature is highest. The allowable temperature rise for comfort is practically zero, whereas the boundary-layer temperature rise in present jet fighter records exceeds 60° F. Such conditions would of course be intolerable for sustained flight, but the duration of flight at record speeds is very short and the temperature in the cockpit has no time to reach equilibrium. Even

The McDonnell XF-88, a twin-jet fighter.

TABLE 1

Altitude, Feet	Temperature, °F.
5,000	41.2
10,000	23.4
15,000	5.5
20,000	−12.3
25,000	−30.1
30,000	−47.9
35,000	−65.7
Above 35,000 and up to 100,000	−67.0

TABLE 2

Altitude, Feet	Maximum Flight Speed, mph
5,000	430
10,000	540
15,000	640
20,000	705
25,000	795
30,000	865
35,000	925
Above 35,000 and up to 100,000	930

for flights of very short duration, however, the record pilot has to put up with a substantial amount of discomfort.

As airplane speeds are increased, a time will come when air conditioning by refrigeration will be essential, but the space, weight, and power requirements of such an air-conditioning system for the temperature control of a limited air volume represented by the cockpit and the passengers' cabin will probably offer no major difficulties in most cases. But as speeds increase still further, structural problems begin to appear. Present electrical and hydraulic equipment is not designed to operate at temperatures much above 180° F. At the same temperature, the strength of polysterene (which is one of the best heat-resisting plastics) is barely 50 percent of its strength at standard room temperature. A similar 50 percent strength loss occurs in commercial magnesium and aluminum alloys at temperatures of the order of 400° F. Even for parts that do not have to bear any stresses, the temperature limits for practical use are below 600° F. for magnesium and below 700° F. for aluminum.

This is serious because, whereas it is relatively easy to air-condition a passenger space properly, the problem of refrigerating a whole airplane, and particularly the outer skin, is considerably more difficult. With an atmospheric temperature of −67° F. around the airplane, the first thought that would

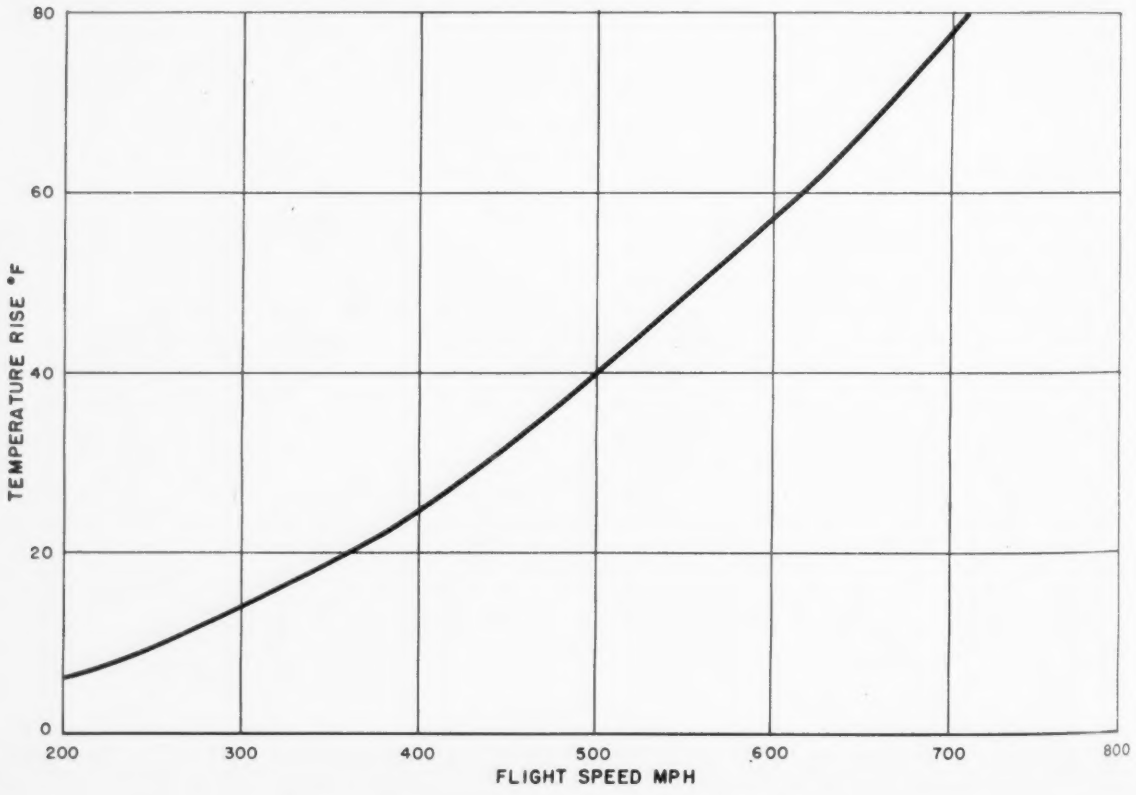


Fig. 1. Skin temperature increase for a typical aircraft.

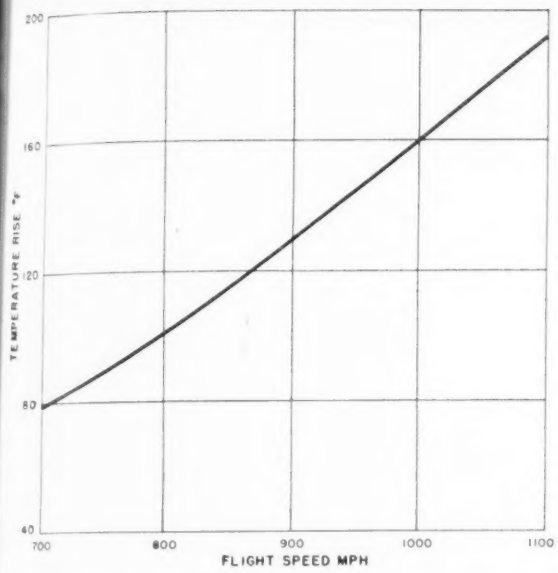


FIG. 2. Showing skin temperature increase for a supersonic aircraft.

come to the layman would be to try to suck in air from the outside to cool the structure from the inside. But, the cooling effect of air being proportional to its mass, because of the air rarefaction at high altitudes, the volume of air required for cooling would be extremely large, and the necessary pumps and pipes of tremendous size.

For flight in the supersonic region at speeds ranging between 1,000 and 2,000 mph in the coldest region of the atmosphere, which incidentally is reached at an altitude of the order of 35,000 feet, a substantial number of engineering problems would appear in specification changes and in the replacement of present standard materials by heavier or costlier, but better heat-resisting, materials. Above 2,500 mph, however, aerodynamic heating is so intense that new structural materials will have to be developed before long-duration flight under such conditions can be sustained. In the language of missile designers, speeds of 4,000 feet per second (2,728 mph) are conservative figures. Firing velocities of this order of magnitude have long been commonplace for naval artillery shells. For short-range missiles, the wall temperatures do not usually have the time to reach equilibrium conditions, and the point of failure of the material is avoided. Artificial skin cooling by evaporation may be taken advantage of, but its use is limited by the amount of liquid that may be evaporated. Considering only the thermal problem involved, increasing substantially the volume of the evaporating medium generally means increasing the over-all volume of the missile and therefore the amount of skin surface to be cooled. Protective refractory and insulating coatings may be considered, but an insulator can slow down the heat transfer only toward the inside, it does not stop it. The same equilibrium conditions will prevail as without an insulator, but the time duration to reach the equilibrium will be increased.

All the above seems to preclude the possibility of space travel, because the flight durations involved would be such as to allow the thermal equilibrium to be established. This, however, would be an incorrect conclusion. The results presented so far are valid only for atmospheric flight. At altitudes of the order of 400,000 feet, air can no longer be considered as a continuous medium, and the mechanism of aerodynamic heating begins to present an entirely different aspect. Heating is produced by bombardment of the moving body by individual molecules, which yield only a portion of their kinetic energy. Its evaluation is beyond the scope of this article, but it can be stated that mo-

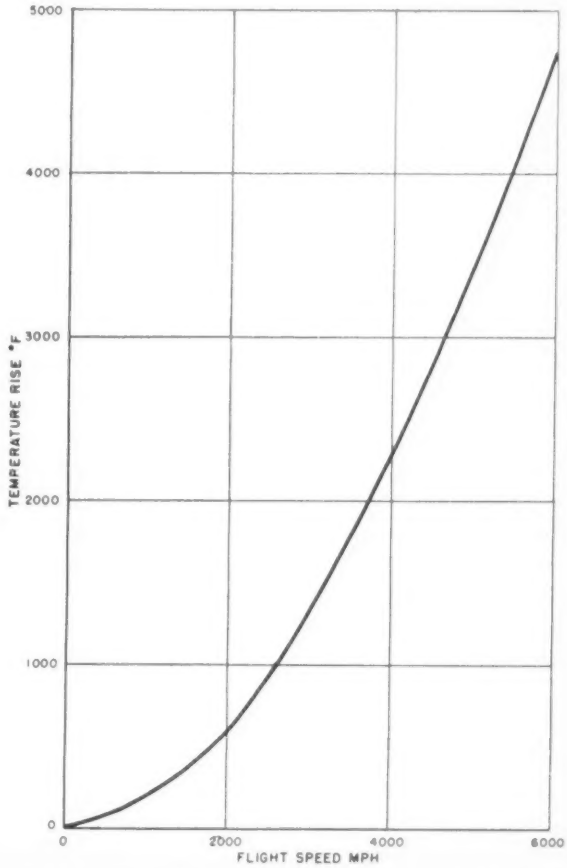
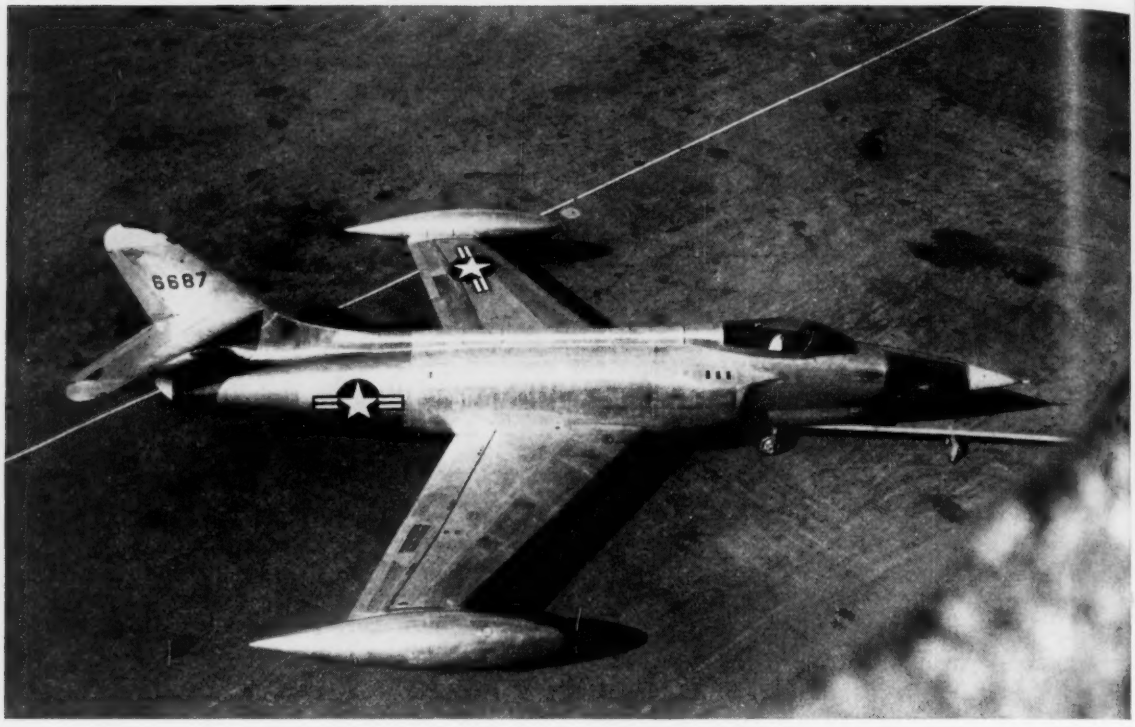


FIG. 3. Skin temperature increase for a high-speed guided missile.



The Lockheed XF-90. A penetration fighter.

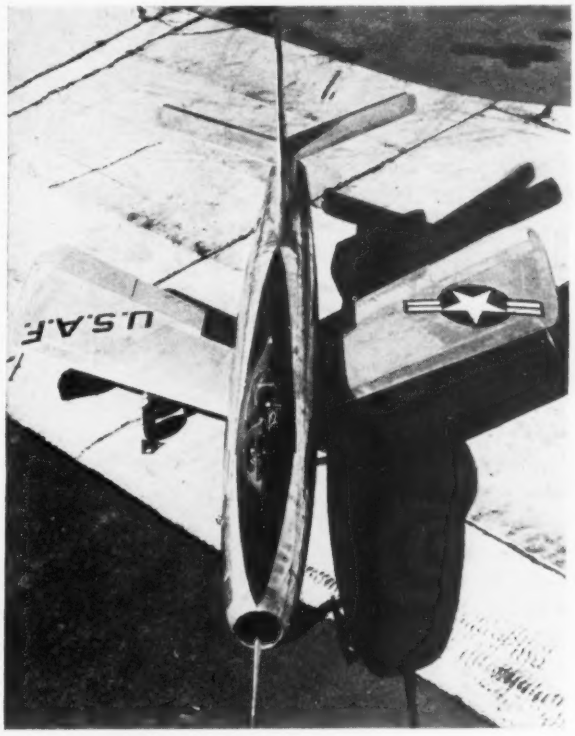
lecular heating at those extreme altitudes is very much smaller than the aerodynamic heating at similar speeds in the lower range of the atmosphere. As the altitude increases, the number of molecules per unit volume decreases; so does the number of collisions between the gas molecules and the moving body, and therefore the heating effect. The angle of attack—that is, the angle between the surface exposed to molecular heating and the direction of flight—is a very important parameter. To reduce heating, it is advantageous to have a very elongated shape, with the smallest possible angles of attack at all points; that is, a high "fineness ratio." To give a simple numerical illustration, at an altitude of 400,000 feet (or roughly 75 miles), the surface temperature of a thin flat-shaped body flying at zero angle of attack would actually be lower than the ambient air temperature prevailing at this altitude (which appears to be of the order of 200° F.) for all velocities up to 20,000 feet per second (or approximately 13,600 mph) if the heat radiation emitted by the body is taken into account. By simply shifting the position of the body so as to make an angle of 20 degrees with the flight path instead of being parallel to it, the body temperature would assume an almost immediate rise of the order of 600° F. at 13,600 mph.

The heating by molecular bombardment decreases rapidly as the altitude increases further, and at an altitude of 800,000 feet, or roughly 150 miles, it becomes negligible, any heating due to or characteristic of the motion disappears for all practical purposes, and the surface temperature of the body is mainly a function of the intensity of the solar radiation. As it has been calculated that at 150 miles altitude the heating by solar radiation would raise the surface temperature of the body by approximately 500° F. (from a temperature of the order of – 250° F.), its effect is of primary importance, but because of its static nature it is beyond the scope of the present discussion.

From all the above considerations, it appears that aerodynamic heating will not stand in the way of intercontinental or space travel. It will, however, impose certain conditions upon the trajectory, limiting the velocity as a function of the altitude, especially in the lower strata of the atmosphere. It may impose the use of refrigeration air conditioning, changes in material specifications, and require the development of new engineering materials. On the other hand, it may be a wonderfully cheap source of heat under conditions where heat may be required. Because the human body is sensitive to relatively very small differences in temperature, the effect of aerodynamic heating has

The Northrop XF-89. Night and all-weather jet fighter.

to be determined with great accuracy. It is a new field of research, open to both theoretical analysts and experimenters, which will probably grow rapidly as technological advancements in aircraft and guided missiles extend the ranges of flight speeds. To name a few of the research organizations involved, the National Advisory Committee for Aeronautics and the Naval Ordnance Laboratory have started an extensive research program to analyze the mechanism of, and collect experimental information on, the aerodynamic heating and the heat transfer between a body and a gas flow at high subsonic and supersonic speeds; and the University of California, under Navy contract, is investigating the same phenomena under conditions of highly evacuated flow. A thorough knowledge of the aerodynamic heating related to the trajectories under consideration would represent considerable progress along the path that will lead to the realization of the extremely high velocities of space travel in the not-too-distant future.



The Republic XF-91, an interceptor fighter. Equipped with a pilot-ejection seat, pressurized cabin, and refrigeration for the pilot at high speeds.

THE WATER ECONOMY OF DESERT MAMMALS*

BODIL and KNUT SCHMIDT–NIELSEN

Dr. Bodil Schmidt-Nielsen is a graduate of the School of Dentistry of the University of Copenhagen. During the war she was amanuensis and research associate at the School of Dentistry. She did research in microanalytical methods and saliva chemistry and also worked with her father, August Krogh. Dr. Knut Schmidt-Nielsen (Ph.D., Copenhagen, 1946) attended the University of Oslo in his native Norway before going to the University of Copenhagen for graduate work on ionic transport. From 1941 to 1947 he held Danish and Norwegian university fellowships. Dr. and Mrs. Schmidt-Nielsen are now assistant professor of histochemistry and research associate, respectively, at the University of Cincinnati.

THE most striking environmental factors in the desert are the heat and the scarcity of water. It is well known that man and many other mammals cannot survive for any length of time in a hot desert without large supplies of drinking water. Figure 1, taken from Adolph's recent book, *The Physiology of Man in the Desert,* illustrates this fact. The graph, which is somewhat simplified, gives the number of days a man will survive in North American deserts. The line running up along the coast gives the limit for a survival time of five days. There is a line for three days' and one for two days' survival; the little encircled area has a survival time of one day only.

In spite of the poor performance of man in the desert, the desert is inhabited by a large number of animals. The problem of how these animals get along without visible sources of water has excited interest for many years.

Dill, in his book *Life, Heat, and Altitude,* divides animals that can live under desert conditions into two groups. First are animals such as the camel, wild burrow, antelope, and several others that can endure the absence of water for a limited time. They must be able to go to sources of water periodically in order to restore their water content. These animals solve their water problem mainly by ability to withstand dehydration and to store water. In the second group are those animals that require no more water than that contained in their food. Some animals feed on succulent plants found in the desert, but certain other animals live on entirely dry food and must conserve water by every possible means.

Some rodents of the family Heteromyidae belong to the second category. The following study of these animals is based on field work in the summer of 1947 and in the late spring and early summer of 1948 at the Santa Rita Experimental Range, Arizona. This is right on the line of two days' survival for man, as indicated by an arrow on the map (Fig. 1). In this area a little pocket mouse (*Perognathus baileyi*), the bannertailed kangaroo rat (*Dipodomys spectabilis*), shown in Figure 2, and Merriam's kangaroo rat (*Dipodomys merriami*)

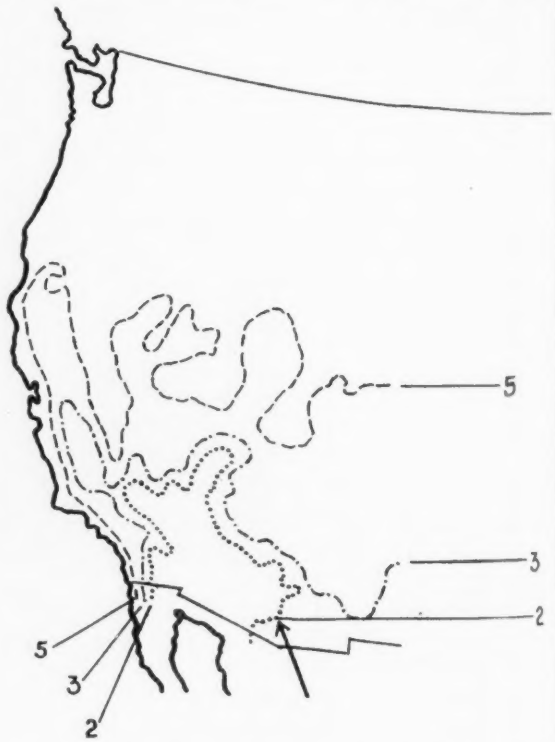


FIG. 1. The number of days man can survive without drinking water in American deserts (simplified from Adolph: *Physiology of Man in the Desert*). These areas are inhabited by numerous animals that live independently of drinking water.

* From a paper presented at a meeting of The Society of the Sigma Xi, Davis, California, April 7, 1949. The work was supported by Contract N7-ONR-380/T.O. 1 between the Office of Naval Research and Dr. L. Irving, Swarthmore College, Pennsylvania.

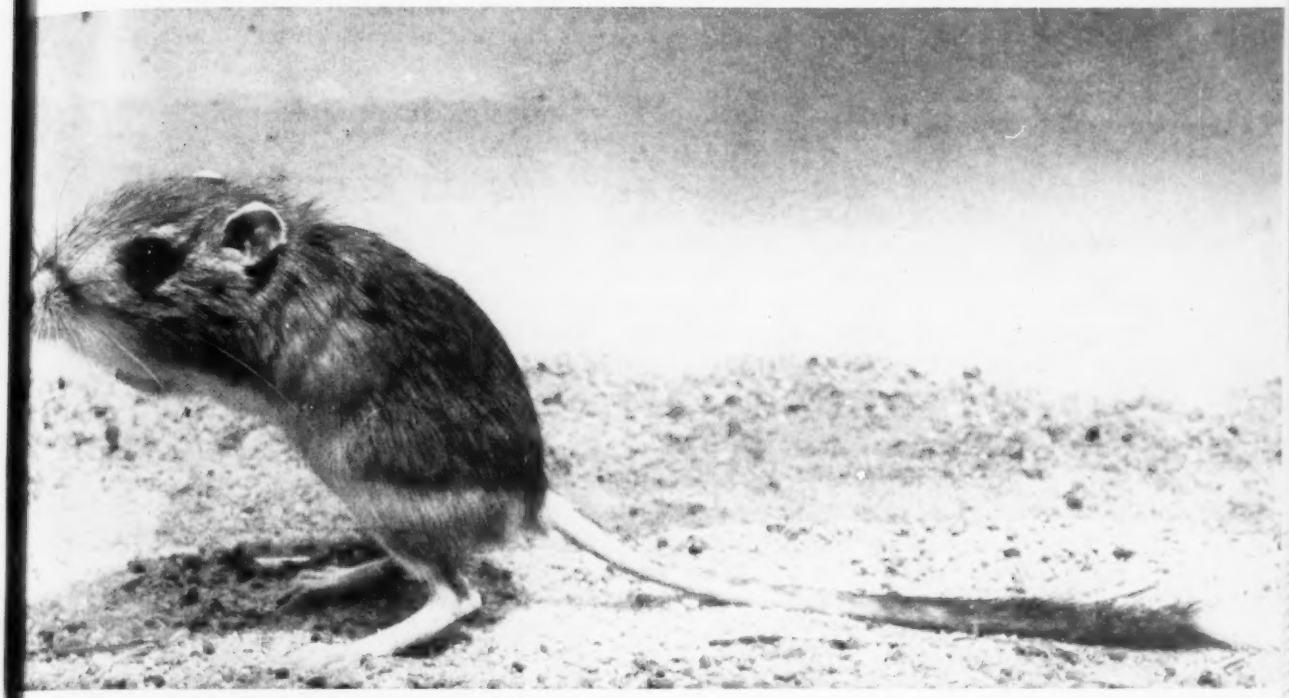


FIG. 2. Bannertailed kangaroo rat from the Santa Rita Range, Arizona, photographed in the laboratory.

riami) were abundant. These are small rodents, with fur-lined cheek pockets. The kangaroo rats look a little like kangaroos, with their elongated hind legs and short, weak forelegs; they also jump like kangaroos. They have large eyes and are nocturnal in their habits, which is of great importance in their water economy, as discussed later. They live in underground burrows.

The bannertailed kangaroo rats build large mounds, with many entrances and a complicated, labyrinthine net of runways. Their nest chamber is at the end of a blind tunnel about two feet under the surface. They keep large stores of food, mainly dry seeds, in their burrows.

The two other species occupy simpler dwellings, with only a few entrances and runways, and they store little or no food. Their nest chamber is about one foot underground. These animals are known to feed nearly exclusively on air-dried seeds and other dry plant material. They do not drink water because they have no access to it, and they do not seem to need it. They do not feed on cactus, as some other rodents do, and they seldom eat green leaves.

ABILITY TO LIVE WITHOUT WATER

In order to make sure that these rodents are able to live on the dry food in the dry desert climate, we kept a large number of kangaroo rats and pocket mice on a diet of dry rolled barley. At intervals of a few weeks, we sacrificed a number of them, weighed them, and determined body water content and serum concentration of urea and salts. The rats did not lose weight on the dry diet, except in the very beginning; after a period of eight weeks, nearly all of them had, in fact, gained weight. Some animals which we kept on the dry grain diet for two and a half months were in fine condition and had all gained weight at the end of the experiment.

The serum concentrations of urea and salt were normal in all the animals that had lived for different lengths of time on the dry diet, and the total body water content did not decrease. All this shows that the heteromyids are able to maintain a positive water balance under desert conditions when they live on a diet consisting exclusively of dry grain. This is a very unusual performance for a mammal, and it must involve specific physiological mechanisms.

If we set up an account for the water balance, we have:

Intake: Drinking water
Water content of food
Metabolic water

Output: Urine
Evaporation from lungs
Evaporation from skin
Feces

As mentioned before, the animals did not get any

drinking water, and the water content of the food was very low. The water content of grain at the humidity in the desert is about 5 percent. The metabolic water amounts to 0.6 g per gram carbohydrate combusted. The whole question of how much water the animals form by their metabolism and lose, simultaneously, by evaporation from their lungs, is very important, and we shall return to it later.

In order to get along on the little water it can derive from its food, the heteromyid will have to conserve water to a high degree. By concentrating the urine, the water loss can be decreased, but a limitation is imposed by the ability of the kidneys to concentrate the urine with respect to salts and urea. The higher the ability of the kidneys to concentrate the urine, the more water the animal will be able to save. We could, therefore, hope to find that the heteromyids are able to excrete a very concentrated urine. Analyses of the urine were, then, one of the important parts of the program when we set out to work on the water metabolism of desert animals.

Water is also lost by evaporation from the skin and the lungs. In some mammals, such as man and the horse, the evaporation through the skin is considerable. There are numerous sweat glands over most of the body surface, and these animals use a large amount of water in their heat regulation. Dogs and rats are supposed to have very few, or no, sweat glands, and the water loss from their skin is, consequently, much lower than in sweating animals. Dogs, on the other hand, regulate heat by hyperventilation and a resulting high evaporation from the respiratory tract.

The kangaroo rats and pocket mice apparently have practically no evaporation through the skin. They do not need water for heat regulation when they live their normal life, because they simply stay away from the heat. We noted in our desert laboratory that the animals died within a short time when the room temperature was around 37° C.

The evaporation from the lungs cannot be avoided. The air that comes into the alveoli will leave the lungs saturated with moisture slightly below body temperature. The amount of water lost from the lungs will depend on the ventilation of the lungs and the amount of moisture in the inspired air. There are two theoretical possibilities for decreasing the evaporation from the lungs: One is to breathe air with a relatively high moisture content, and another is to decrease the ventilation of the lungs. The heteromyids seem to utilize both these possibilities.

The water content of the feces is quite small, the feces of the heteromyid being very dry. From 100 grams of barley they produce about 5 grams of feces with less than 50 percent water content.

The two main sources of water loss from the heteromyids are, therefore, the urine and the evaporation from the lungs.

WATER EXPENDITURE FOR URINE FORMATION

The urine of the heteromyids is excreted in minute amounts. To analyze it, we used micromethods for the determination of total electrolyte content, urea concentration, and chloride concentration. All these determinations could be made on a total of 0.01 ml or less. We analyzed urine from heteromyids that had just been trapped and from animals that had lived for some time on the dry grain diet. The results immediately revealed that the heteromyids can concentrate their urine to an amazingly high degree.

The maximum urine concentration found in man is about 0.3 N, or 2 percent, of electrolytes, and 1 M, or 6 percent, of urea. The Norway rat has been supposed to be one of the mammals with the most efficient kidneys. It can excrete a urine that is 0.6 N, or 3.5 percent, with respect to electrolytes; and it can execrete urea in concentrations up to 2.5 M, or 15 percent. Maximum concentrations in the heteromyid urine, when the animals lived on the dry grain diet, were found to be 1.1 N for electrolytes and 3.8 M for urea (Table 1).

In order to get information regarding the physiological capacity in general, and the maximum performance of the renal execretory system in particular, we imposed extra loads of protein or salt on the animals by giving them diets containing an excessive amount of protein or sodium chloride, respectively. For a high-protein diet we used dry soy beans; for a high salt diet, barley soaked in a salt solution and dried. The grain thus treated contained 10 percent of sodium chloride by weight.

The animals could not survive on these diets; they lost weight and died within three weeks. But the urine they excreted was highly concentrated. We found values for urea concentration up to

TABLE 1

MAXIMUM CONCENTRATIONS OF ELECTROLYTES AND UREA IN URINE

	Electrolytes	Urea
Man	0.37 N (2.2%)	1.0 M (6%)
Norway rat	0.60 N (3.5%)	2.5 M (15%)
Heteromyids	1.2 N (7%)	3.8 M (23%)

3.8 M (23 percent). Electrolytes were also excreted in very high concentrations, up to 1.2 N. This is twice as concentrated as sea water and is nearly twice the maximum excretory ability known for other mammals. Furthermore, we found very high electrolyte concentrations simultaneously with very high urea concentrations. Making a rough estimate of the osmotic value of such a urine sample, we find that it is just about six osmolar, an amazing value compared with what is to be found in other mammals.

It is generally believed that mammals like man and the rat can only increase the urea concentration at the expense of salt concentration, and vice versa. It is difficult to understand how the kidneys of the heteromyids can perform such enormous osmotic work, but it is obvious that they have a very useful mechanism for water conservation. A heteromyid needs only half the amount of water a white rat needs for excreting its waste products.

With such high urine concentrations, heteromyids should actually be able to gain water when drinking sea water. In order to show that they really are able to do so, a special experiment was performed. A number of kangaroo rats were divided into three groups and given the following diets:

Group 1, dry soy beans
Group 2, dry soy beans and sea water
Group 3, dry soy beans and fresh water

The animals in the first group lost weight and died within three weeks. The animals in the second and third groups lost weight during the first two or three days, until they learned to drink. Then they all gained and increased to above their initial weights.

WATER LOSS THROUGH EVAPORATION

In order to determine the order of magnitude of the evaporation from the lungs, measurements of relative humidity and temperature in and around the burrows of the animals were necessary. Vorhies, during a whole year, recorded the temperature inside and outside the burrow of a *Dipodomys spectabilis*. He also recorded the humidity outside the burrow, but he did not have any suitable hygrometer with which he could record the humidity inside the burrow. For this purpose we used Krogh's microclimate recorders, which record temperature and relative humidity for a twelve-hour period. The record is made on a smoked circular glass disc, which can be read afterwards under the microscope. The humidity-sensitive element in the recorder is a hair hygrometer. It has, therefore, the same limitation as other hair hygrometers, in that it will not record humidities much below 20 percent with any accuracy. In the beginning, we did not believe that this recorder would be very useful in our work, because we expected relative humidities to be lower than 20 percent inside the burrows. But a brief survey showed that the humidity in the ground and in the burrows ranged much higher than we had anticipated, so the recorders turned out to be extremely useful.

In order to get the recorder down into the burrow, we tied it to the rat's tail, and released him in front of his own burrow. He would then drag the recorder down into the nest chamber. To prevent the animal from running away with the recorder, we had a thin wire tied to the instrument and fastened outside. After twelve hours we excavated the burrow. Table 2 summarizes the results. We found some variation from burrow to burrow, but the humidity was always considerably higher inside the burrows than outside. The humidity outside the burrows was sometimes extremely low—humidities below 5 percent relative humidity were frequently measured. We see, therefore, that it is most advantageous for the animals to stay in their burrows, instead of running about in the dry desert.

TABLE 2

APPROXIMATE HUMIDITY OF *Dipodomys* BURROWS AS COMPARED WITH THE OUTSIDE AIR. EARLY SUMMER IN ARIZONA DESERTS

	Temperature, Degrees C	Relative Humidity, Percent	Absolute Humidity, mg H_2O/ Liter Air
Inside burrows	25–30	30–50	8–15
Outside burrows, day	20–45	(1)–15	1–5
Outside burrows, night	15–25	15–40	2–5

We can calculate how much water the animals save by staying in the burrows. We can assume that *Dipodomys*, like other mammals, expires air that is saturated with moisture slightly below body temperature. Normal body temperature for *Dipodomys merriami* is 36°–37° C, and the following calculations were made under the assumption that the expired air has a moisture content corresponding to saturation at 33° C (Dill, p. 26). Air saturated with water vapor at 33° C contains 35.3 mg of water per liter air. A *Dipodomys* that is breathing completely dry air will, therefore,

lose about 35 mg of water per liter of air expired. If the animal breathes desert air with a moisture content, as we measured it during the daytime, of about 2 mg water per liter air, it will lose approximately 33 mg of water per liter air expired. If, however, the animal stays in its burrow, the air it inspires will contain about 10 mg of water per liter, and the animal will lose only about 25 mg of water per liter expired air, or 24 percent less than in the first case.

It is obvious that a high moisture content of the inspired air results in a lower evaporative loss from the lungs, and that the absolute humidity and not the relative, is the determining factor. In this way a relatively dry and warm burrow may be more advantageous than a cold and relatively humid burrow, because the latter may have a lower absolute humidity.

In the water economy of the heteromyids, this saving is very important, because it determines whether the production of metabolic water will lead to an ultimate loss or gain. A common view is that metabolic water is of course useful, but not a very decisive factor, since the metabolism, in addition to formation of water, involves respiration, i.e., evaporation from the lungs.

Howell and Gersh (1935) are of the opinion that the animal will always lose more water by evaporation from the lungs than will be formed by the metabolism, so that an increased metabolism in order to form more water is too expensive to be practicable. We cannot quite agree with this viewpoint. The evaporation from the lungs is influenced by the humidity of the inspired air and the ventilation of the lungs, and whether the formation of metabolic water will lead to an ultimate loss or gain can easily be calculated.

The amount of water formed per gram of foodstuff combusted is shown in Table 3. In the same table is also shown the amount of oxygen used per gram foodstuff combusted; and, finally, the amount of water formed per ml of oxygen used is calculated.

TABLE 3

AMOUNTS OF WATER FORMED AND OXYGEN USED WHEN PURE FOODSTUFFS ARE METABOLIZED

	Carbohydrate	Fat	Protein
Grams of water formed per gram foodstuff combusted	0.556	1.071	0.396
Liters oxygen used per gram foodstuff combusted	0.80	2.01	0.95
Mg water formed per ml of oxygen used	0.69	0.53	0.42

Using the figures for the composition of grain from a nutritional chart, we can calculate the amount of water formed per ml of oxygen used when grain is metabolized (Table 4).

Next, the figures for the amount of water formed by the metabolism are compared with the figures for the amount of water lost by evaporation from the lungs. With 16 percent oxygen in the expired air (normal value for man and most other mammals), 50 ml oxygen are taken up per liter air expired. When corrected for temperature, vapor pressure, and barometric pressure, this gives 42.4 ml O_2. If the animal breathes completely dry air, it will lose 35.3 mg of water per 42.4 ml oxygen taken up, or 0.83 mg of water per ml oxygen. This figure is larger than the figures shown in Table 4 for the amount of water formed per ml oxygen used when grain is metabolized. This means that an animal breathing dry air will lose more water by evaporation from its lungs than is formed by its metabolism. In other words, for the conditions stated, the formation of metabolic water will lead to an ultimate loss of water.

TABLE 4

AMOUNT OF METABOLIC WATER FORMED FROM THE COMMON TYPES OF GRAIN, CALCULATED IN RELATION TO THE AMOUNT OF OXYGEN REQUIRED FOR THE COMBUSTION OF THE FOOD IN THE BODY

	mg H_2O/ml O_2
Barley	0.66
Oats	0.63
Wheat	0.62

If the animal breathes air containing 10 mg of water per liter (as is found in the burrows), the loss of water from the lungs will be 0.60 mg per ml oxygen. In this case, water is gained by the metabolism and an increase in the metabolism will lead to an increase in the water gained.

In the dry season, the moisture content of the air outside the burrows during the night was about 2–5 mg of water per liter air. In that air the water loss from the lungs will be about 0.7 mg per ml oxygen. The amount of water gained about equals the amount of water lost. As the animal spends less time outside the burrow than inside, the over-all picture will show a gain in water by the metabolism.

This calculation, however, was built upon the assumption that the ventilation of the lungs is the same as in man and that the expiratory air is

saturated with water. In order to find out whether the evaporation from the lungs can be decreased below this level, we measured the evaporation from the animals simultaneously with the oxygen uptake. We found that the kangaroo rats evaporate less water per ml of oxygen used than the ordinary white rats. Twelve heteromyids evaporated on the average 0.53 mg of water per ml oxygen taken up, whereas 10 white rats evaporated on the average 0.93 mg water per ml oxygen. If we compare these figures with the theoretical figure for the water loss from the lungs when completely dry air is inspired (0.83 mg H_2O per ml O_2), we see that the kangaroo rats show considerably lower values. In other words, the kangaroo rats are able to reduce the evaporative water loss from the lungs, probably by decreasing the ventilation of the lungs.

Summarizing the results of our studies, we can say that the desert rats, as Dill predicted, really do save water by every possible means. They spend no water for heat regulation. They excrete a highly concentrated urine. They have a lower evaporation of water than white rats. They further decrease the evaporation from the lungs by staying in their more humid underground burrows.

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INTERNATIONAL PHOTOGRAPHY–IN–SCIENCE SALON

Judges in the Third Annual International Photography-in-Science Salon, which is sponsored by The Scientific Monthly in cooperation with the Smithsonian Institution, will be: Dr. Merle A. Tuve, of the Carnegie Institution, for the physical sciences; Dr. Ronald Bamford, of the Department of Botany, University of Maryland, for the biological sciences; A. A. Teeter, recently of Charles Pfizer & Co., New York City, for chemistry; Dr. Emanuel Krimsky, of Polyclinic Hospital, New York City, for the medical sciences; and Alexander J. Wedderburn, of the Graphic Arts Division, Smithsonian Institution, for photography.

Entries in the competition, which was established to encourage and extend the use of photography as a basic research tool, will be received by the Editor of The Scientific Monthly, 1515 Massachusetts Ave., N. W., Washington, D. C., from August 24 to September 14, 1949. Winners will be announced on or before October 1.

First, second, and third awards and five honorable mentions will be given in each of two divisions, black-and-white and color. The Judging Committee will consider the initiative, originality, and results obtained more than the composition and pictorial quality. All photographs must be taken for scientific purposes.

The prints selected for awards and display will be shown at the U. S. National Museum during October 1949, and at the New York Meeting of the AAAS, December 26–31, 1949. They will then go on a tour of important scientific institutions in this country and abroad.

THE FATE OF MORGAN'S BEAVER

RICHARD H. MANVILLE

Dr. Manville (Ph.D., Michigan, 1947) is assistant professor of zoology at Michigan State College and well acquainted with the area of which he writes.

FROM the time of Hearne's first account[1] in 1795 until the recent article by Gregg[2] in THE SCIENTIFIC MONTHLY, the beaver has had his full share of publicity in the literature of North American mammals. The largest of our native rodents, with many specializations for his peculiar type of aquatic existence, he is renowned alike for his works as a forester and landscape architect, for his engineering ability, and for his social organization. Indeed, an almost human degree of intelligence has frequently been imputed to him. His pelt lured the early trappers westward and was largely instrumental in opening up the continent. The beaver has figured prominently in folklore, legends, and children's tales; he is the subject of volumes by Dugmore,[3] Johnson,[4] Martin,[5] Mills,[6] and Warren,[7] and of innumerable shorter articles. Amidst this mass of fact and fiction, perhaps no more penetrating study of the beaver has been made than that by Morgan;[8] it is to this day a classic in its field.

The discovery of iron in the Marquette range of Michigan's Upper Peninsula, in 1844, was a tremendous boon to this backwoods country. The first iron ore was mined near Negaunee in 1846; the following year the first company for mining iron was organized. The Carp River forge was

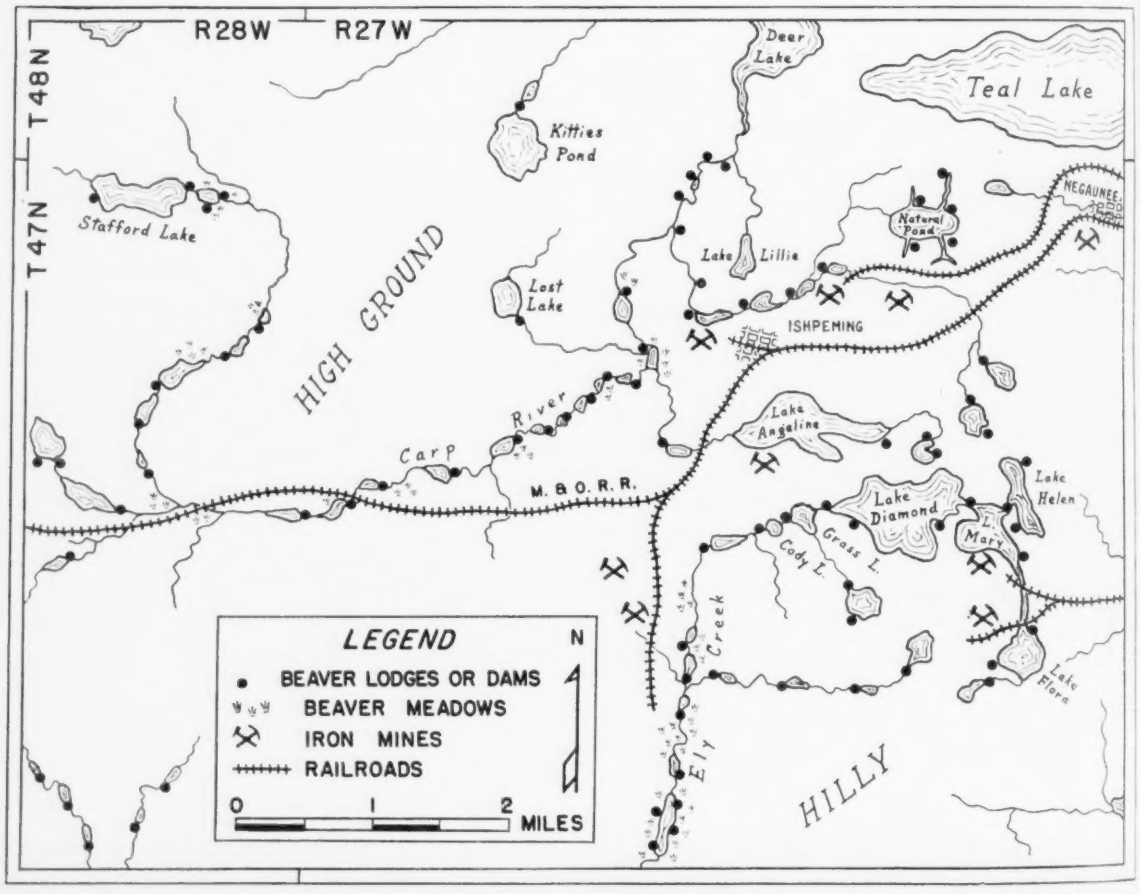

FIG. 1. The area of Morgan's observations in Marquette County, Michigan, about 1867. Modified from Morgan's map.

erected in 1847 and produced the first iron early in 1848. As a director of the recently developed Marquette and Ontonagon Railroad, Lewis H. Morgan had occasion to visit the country while it was yet a wilderness. Attracted at first by the angling possibilities, then intrigued by his casual observations of beaver workings, he soon developed a deep interest in the animal—so much so that he devoted most of the summers from 1855 to 1867 to its study in this area. He was a keen observer, a careful recorder of what he saw, and he presented a picture of beaver life in a lucid, distinctive style. So clear were his maps and descriptions, in fact, that in some cases it is possible, even now, to locate the exact spots where his observations were made, as I did in August 1948. The present remarks are chiefly of historical value and may impress upon us the importance of man as an ecologic factor in the community where he dwells. An appraisal of the situation as regards the beaver, some ninety years after Morgan's observations, may be of general interest.

Morgan's studies were concentrated largely in an area of some forty-eight square miles (Fig. 1). Nine iron mines had been sunk within the area; the railroad had but recently pushed across the country, reaching Ishpeming in 1858; and the only settlements established in the wilderness were the hamlets of Ishpeming and Negaunee. Scattered lakes and ponds and many small streams, draining principally into the Carp and Escanaba rivers, made an ideal situation for the existence of beavers. The lands were heavily forested, with a plentiful supply of aspen, the beavers' favorite food, adjacent to the waters. The courses of Carp River and Ely Creek were studded with beaver meadows, the scenes of earlier beaver ponds that had filled with sediment and grown up to grassland. Throughout this region Morgan mapped the beaver dams, lodges, and meadows and located at least eighty sites of beaver activity.

The picture today is a vastly different one (Fig. 2). On careful scrutiny the country may be recognized as the same, despite changes in the names of many of the lakes and ponds and corrections of some of the minor inaccuracies in Morgan's map.

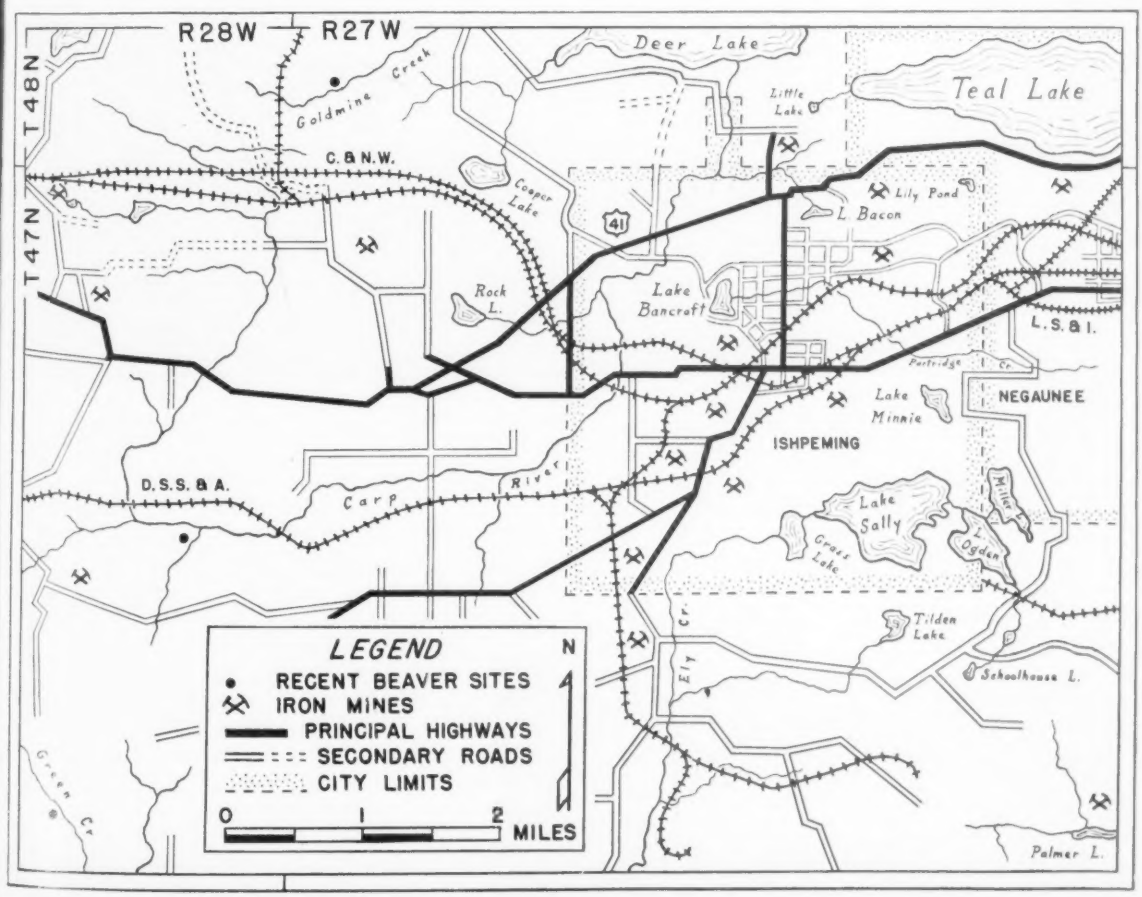

FIG. 2. The same area as shown in Figure 1, in 1948.

FIG. 3. The western end of Teal Lake, 1948, approximating the primeval conditions.

FIG. 4. The eastern end of Teal Lake, 1948, showing mining structures of the Cleveland-Cliffs Iron Company.

FIG. 5. The Carp River, 1948, as seen from highway U.S. 41 east of Teal Lake.

FIG. 6. A portion of the beaver dam at Grass Lake in August 1861, as illustrated by Morgan. (Used by permission of J. B. Lippincott Company.)

FIG. 7. The same site at Grass Lake in August 1948.

FIG. 8. A view across Grass Lake in 1948 from the bluff shown in Figure 7.

The beaver meadow succession has continued, to the point of forests in many cases, so that most of the ponds mapped by Morgan are no longer in existence. Others—for example, Stafford Lake, Lake Flora, and Kitties Pond—are much reduced in size. The configuration of Deer Lake has been greatly altered by impounding; Lake Angeline has been drained as a consequence of mining operations. Ishpeming and Negaunee (populations 9,491 and 6,813, respectively) have spread over much of the country. Three railroads, a Federal trunk highway, and a network of lesser roads penetrate to most parts of the region. Some of the mines are no longer in production, some are represented by huge areas of caved-in ground—but many are operating with renewed vigor. Forests have been cut, waters polluted, and the beavers themselves persecuted by trappers. The wonder is that, with conditions thus altered, the beavers persist at all in the more remote parts of this country where Morgan once found them so plentiful.

Teal Lake, although not studied in detail by Morgan, typifies the changes that have occurred. Today the western end of this beautiful lake (Fig. 3) is relatively unmodified, and deep forests still fringe its shores. A main highway, however, skirts its southern shore, and at the eastern end (Fig. 4) a settlement has grown up and mining structures rise above the horizon. Beavers have disappeared from Teal Lake.

Along the Carp River and its tributaries Morgan noted no less than forty sites of beavers' activity. In streams as large as this, bank burrows were frequently constructed in addition to the more orthodox lodges of sticks and soil. Dams, which were often built across small streams, were impractical here because of the threat of spring freshets and flash floods. Many canals were dug by the beavers along the Carp River, often at bends in the stream, to lessen the task of hauling branches for food and structural work. But beaver activity along the Carp River, once so prevalent, is now restricted to a few small tributaries at its headwaters. Elsewhere, although the forest cover is in many places undisturbed (Fig. 5), the river is polluted by wastes from the mines, and its waters are red with iron sediments.

Morgan's most detailed observations were made in the area about Lake Diamond (now called Lake Sally), which has been raised some ten feet by a concrete dam and serves as a water supply for the city of Ishpeming. No longer do beavers occupy this lake or its outlet, Ely Creek, where Morgan found them so numerous. Not far below Lake Diamond was Grass Lake, a pond of about sixty acres, formed by a beaver dam across Ely Creek. This great beaver dam, described by Morgan (pp. 86, 99) as "the most remarkable of all the structures . . . not surpassed in magnitude by any other beaver dam in North America," measured more than 260 feet in length along its crest and over 6 feet in vertical height at the center of its great curve. Morgan estimated that it contained upwards of 7,000 cubic feet of solid materials. At considerable pains he and his party photographed this dam in August 1861; from the photograph, the engraving (Fig. 6) published in his book as Plate VII was made. Beavers are absent today from this locality and the great dam has completely disintegrated. The marsh succession, evident even when Morgan viewed the scene, has progressed far. The changed nature of the area is shown in Figure 7, a present view from practically the same site as Morgan's illustration. The contour of the hill and the clump of trees at the right are recognizable features. The former location of the dam is marked only by the junction of the terrestria. willows and alders with the lower-growing, more aquatic, vegetation of leatherleaf, bog rosemary, and pale laurel. An elevated view from the opposite side of the pond (Fig. 8) shows clearly how much of Grass Lake is now filled with emergent aquatic vegetation.

Morgan discussed at some length the manner in which beavers fell trees and section them into logs. Usually this is done in a stereotyped fashion, but occasionally unusual types of cutting are observed. One of these, a stump with two deep incisions

FIG. 9. An unusual beaver cutting in the near-by Huron Mountains, 1941.

around it, and severed at a third spot above, was interpreted by Morgan as an indication of the beaver's inclination to eat wood as well as bark, probably in the winter or early spring. Gregg[2] illustrated a spirally gnawed tree in which a stout wire wound about the tree had caused this peculiar type of cutting. Not many miles north of the area of Morgan's study, in the Huron Mountains, I observed, some years ago, a similar example of beavers' work (Fig. 9). This was an aspen stump of about ten inches basal diameter, with two successive cuts encircling it, but only the third severing it. It is my belief that this stump represents winter cuttings of beaver on several different nights, with sufficient snow falling in between to necessitate starting a new cut the next night. At the third attempt, the tree was either felled in one night or snow ceased falling until the operation was completed.

From the evidence of beaver meadows, fallen trees, lodges, dams, and excavated burrows and canals, Morgan concluded that beavers had lived in this country for countless centuries. He went so far as to state (p. 84) that "these dams have existed in the same places for hundreds and thousands of years, and . . . have been maintained by a system of continuous repairs." The Indians of the region, practicing a type of wildlife management of their own, had apparently not been inimical to the beaver colonies. But Morgan foresaw their diminution as a consequence of the establishment of human settlements near by. He wrote (p. 123) that "these dams begin to decay as soon as they are deserted by the beavers, and quickly thereafter disappear; and . . . in no case do the latter remain in any district long after the establishment of the first settlements in their vicinity."

We now know that beavers, if supplied with their normal needs and not unduly persecuted by man, are somewhat tolerant of civilization. Even when greatly reduced, a beaver population exhibits a high degree of resilience if afforded proper protection, as has been demonstrated in Maine, New York, California, and elsewhere. But Morgan's predictions were, on the whole, remarkably astute. Beavers probably now number near a dozen, where Morgan once estimated them by the hundreds. Throughout the entire area, I found recent evidence of their presence only along upper Goldmine Creek, in the northwestern corner of the tract; even here they were absent in 1948, but cuttings indicated that they had not been gone for many years. A summer camper along Goldmine Creek told me of a few living there four or five years before, and stated that in recent years two beaver dams on this creek were blown out. It is possible that a few beavers still persist along Green Creek, in the extreme southwestern part of the area, and there may even be a few inhabiting bank houses along the upper waters of the Carp River. Elsewhere, as foretold by Morgan, their structures have fallen into decay and they themselves have departed.

Advancing civilization has spelled the doom of much of our native fauna, and it is a sad commentary on the white man's culture that he is less able to coexist with the native animals than were his aboriginal predecessors. In the conflict the beaver has fared better than some, and yet the results of human dominance are all too evident. Given a modicum of consideration, this remarkable rodent will thrive in close proximity to man. Much of the area here under consideration is still virtual wilderness, with many streams suitable for beavers. Aspen is plentiful; many trees in the Grass Lake area measure more than twelve inches in diameter. The supposed desirability of the area for beavers is affirmed by the introduction here in 1948, by the state Conservation Department, of several nuisance beavers from country to the east. Their fate I do not know. Apparently it is not simply such features of encroaching civilization as railroads, highways, mines, and towns that are fatal to the beaver population. Rather it is the intentional destruction of the animals by trappers, legally or otherwise, or of their dams by fishermen who consider such dams a menace to their interests. Or, again, it may be the unintentional destruction of beavers' habitats by logging the forests, damming the lakes, or polluting the streams, which the beavers are unable to tolerate. These factors, many of which are subject to man's control if he so desires, hold the key to the future of the beaver. A proper appreciation of the aesthetic appeal of this rodent should enlist the aid of those in a position to assure his future existence in the country where Morgan first observed him so carefully.

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SCIENCE ON THE MARCH

MAGNETIC RECORDING

AMONG the different methods of sound recording, magnetic recording has always occupied a unique position. It is the only method in which no permanent changes in the physical characteristics of the recording medium are brought about by the recording process.

In mechanical recording—for example, in disc recording—the surface of the recording medium is permanently changed by producing a modulated groove. Once surface portions of the recording medium have been removed by cutting, or deformed by embossing, the recording material cannot easily be restored to its original physical condition.

In optical recording, as in sound film, a light-sensitive layer is exposed to a light source of varying width or intensity. This results in a change in transparency of the film after it has been developed. Even more than in mechanical recording, it is difficult to recondition the light-sensitive layer so that it can again be used.

In magnetic recording, on the other hand, the recording medium can be used again and again. A magnetic record is made by impressing a varying magnetic field upon a moving magnetizable material. The magnetic recording medium, like any other magnetic material, does not experience any permanent physical changes after it has been subjected to magnetic fields. There may be temporary effects, such as expansion and contraction, caused by the magnetostriction of the material, but these dimensional changes can always be eliminated by bringing the material back to its magnetically neutral condition. In magnetic recording, because of the nature of the recording method, mechanical vibrations, temperature variations, and light have very little influence on the recording characteristics.

The fact that the physical characteristics of the recording medium are modified owing to magnetization implies that this method imposes basically only such frequency limitations as are usually inherent in electromagnetic processes. In order to record high frequencies of any predetermined value, it is only necessary to create appropriate operating conditions by taking into account the fact that storage of signals always requires space and that, thus, the process of recording or reproducing always requires relative motion of proper magnitude between the recording and playback device on the one hand and the recording medium on the other. Because of such motion, to each frequency a wave length can be assigned, and this wave length λ—that is, the length of the medium required to record one cycle—is given by

$$\lambda = \frac{v}{f},$$

where v is the velocity of the medium and f is the frequency of the signal.

As long as the magnetic recording medium moves fast enough so that the recorded wave length will never be shorter than a predetermined quantity in the direction of the motion of the recording medium, it is at least theoretically as easy to cover a frequency band up to one megacycle as it is to record frequencies falling within the audio spectrum. It has been found that in any practical recording system a wave length of about 0.001 inch is the minimum limit whenever reliable operation is expected. This limitation can also be expressed by stating that for each additional 1,000 cycles per second response versus frequency, the speed of the medium has to be increased one inch per second. If, for example, a 50-kc response is wanted, the medium would have to move with a velocity of 50 inches per second.

What makes it difficult to impress upon the recording medium and to reproduce from it a wave length shorter than 0.001 inch? The attempt to answer this question leads into the fundamental problems of recording. In general, there are three factors that determine the capability of a system with regard to the shortest wave length.

1) The ability of the recording device to affect only an infinitesimal length along the path of the sound track.

2) The degree of resolution of the playback device.

3) The ability of the recording medium to maintain an impression of predictable magnitude at any one point along the path of the sound track.

The closer these three requirements can be met, the more information can be stored within any given length of the sound track. It is therefore important to analyze these three points.

The recording device, usually called the "re-

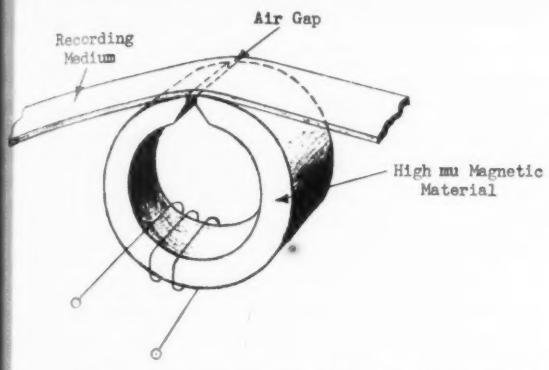

FIG. 1. Typical ring head as used for magnetic recording and reproducing.

cording head," must be so constructed that it will focus the magnetic flux so as to affect only a minimum length along the motional direction of the recording medium. Unfortunately, there is no magnetic insulator available to eliminate a magnetic field where it is unwanted. The best approach so far has been that of using a structure of high permeability material to confine the magnetic field and form the smallest possible air gap within the structure for creating a concentrated magnetic flux where it is needed.

Magnetic heads of a variety of configurations have been built. Lately the trend has been to use ring heads similar to that shown schematically in Figure 1. The length of the air gap over which the recording medium travels cannot, for practical considerations, be made much shorter than 0.0005 inch. The leakage field which is developed around the gap is not limited to the physical gap length but usually extends beyond it.

The effective gap length is not always the factor which controls the smallest wave length that can be recorded. The zone of magnetic influence of the recording head and the "biasing" process have to be simultaneously taken into consideration.

The playback device, usually called the "reproducing head," must be capable of scanning the sound track with the highest degree of resolution. Thus the function of the reproducing head is to detect the magnetic conditions of the recording medium as they exist within an infinitesimal length of the sound track. It can be shown that the flux emanating from such a short portion of the sound track is related to the flux retained within the medium at that "point." The reproducing head is frequently so designed as to guide all external flux lines which are generated over a relatively extensive length of the sound track through an appropriate path, but to evaluate only those flux lines which specify the condition at the point under consideration. This is exactly what is done by the previously described ring head, its effective gap length setting a limit to its resolving power. The reproducing head gap length in magnetic recording has, then, the same effect as the slit width in optical recording.

When the recorded wave length approaches the dimensions of the gap length, the reproducing head cannot continue to perform its function properly. In Figure 2 the loss in the playback process is plotted against the ratio of the effective gap length to wave length. When this ratio equals 1, 2, 3, etc., no response can be expected from the scanning device. With a gap length of 0.0005 inch and an effective gap length somewhat larger, it becomes obvious that wave lengths of approximately 0.001 inch are about the shortest that can be reasonably well handled.

The recording medium must be capable of retaining a magnetic impression in such a manner that the remanent magnetization of one point does not affect the remanent magnetization of another point. This means that the magnetic material must be highly resistive to any change of its magnetic condition. Such resistance to change is essentially given by the coercivity value of the material. Even if it were possible to choose an arbitrarily high coercivity for the recording medium, compromises have to be made for practical considerations. The requirement of being able to alter the magnetic status only under the influence of substantial forces during recording implies that even more substantial forces are needed to restore the recording medium to its original condition, since, as a part of such a process, the material has to be at least temporarily magnetically saturated. Restoring, or as it is more frequently called, "erasing," is performed by the erasing head. It is difficult to design

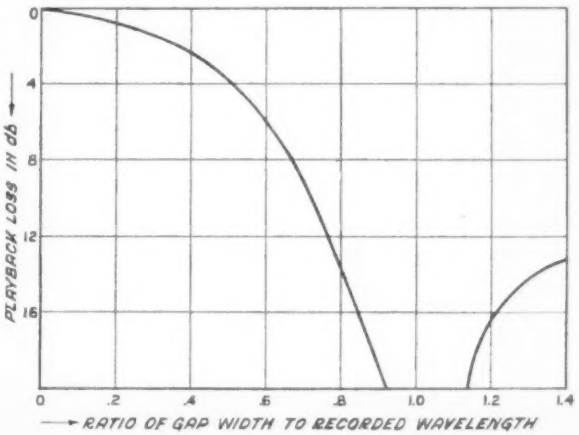

FIGURE 2.

an erasing head generating magnetizing forces in excess of 1,500 oersteds. The strength of the magnetic field of the erasing head sets the limit for the coercivity of the recording medium. A coercivity of 200–300 oersteds is usually considered a satisfactory compromise.

So far, nothing has been said about the relationship of the value of remanent magnetization left in the recording medium and the magnetizing forces of the recording head which are the cause of the remanent magnetization. Although not important for all applications of magnetic recording, for many it is essential that these two quantities be linearly related. Considering the nature of permanent magnetic materials, such linearity cannot easily be obtained. If one attempts to record on a magnetically neutral medium without the use of any other auxiliary means, the remanent magnetization left on the sound track will be badly distorted compared to the magnetizing forces which produced the remanent magnetization (Fig. 3).

A special treatment, which in the lingo of magnetic recording terminology is called "biasing," must therefore be applied. Two methods of biasing are known. The d.-c. method takes advantage of the fact that the up-and-down branch of the hysteresis curve has a long, relatively straight portion of which advantage can be taken in the recording process. A hysteresis curve of a typical magnetic recording material is shown in Figure 4. Prior to

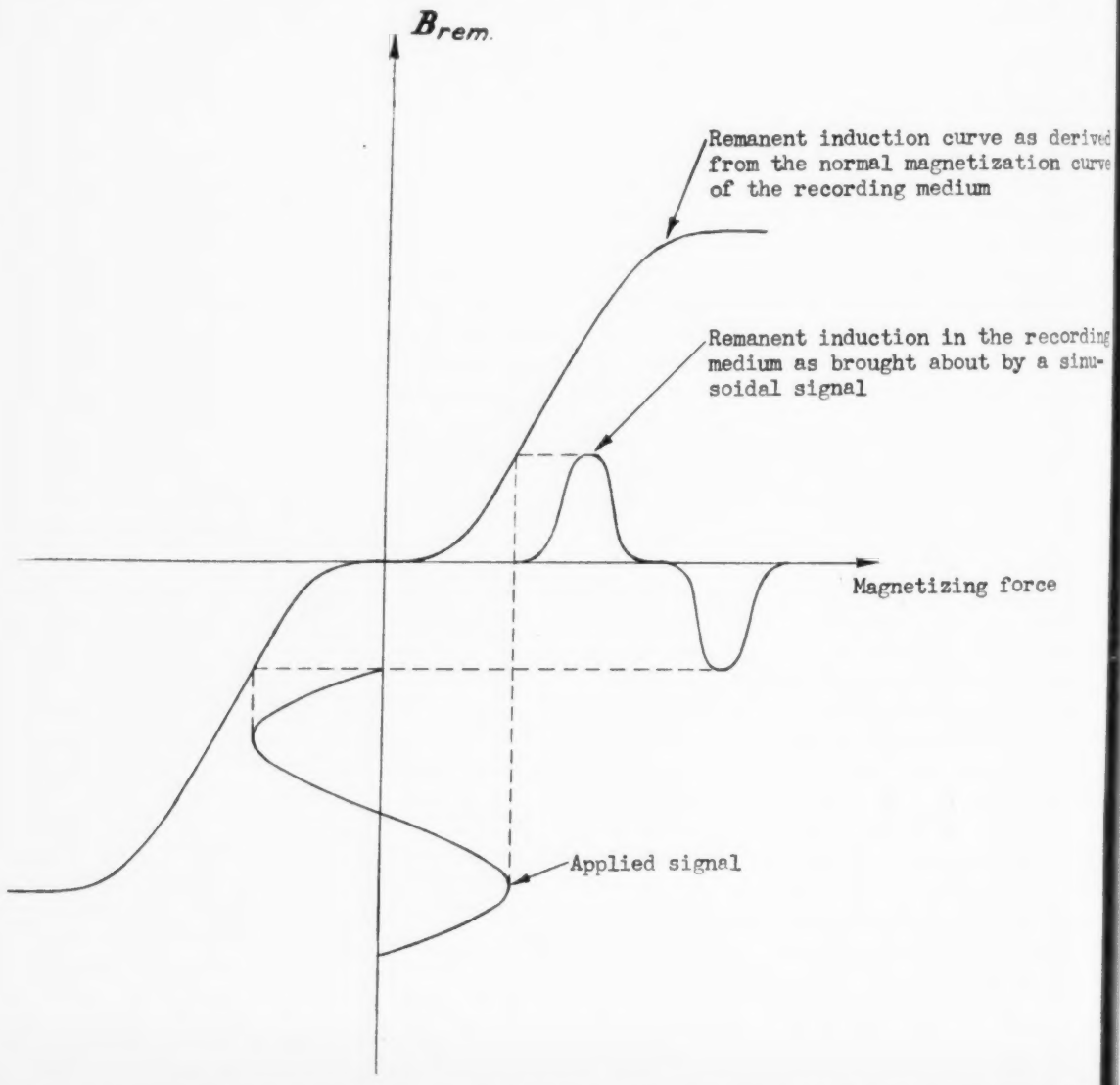
$B_{rem.}$
Remanent induction curve as derived from the normal magnetization curve of the recording medium
Remanent induction in the recording medium as brought about by a sinusoidal signal
Magnetizing force
Applied signal

FIG. 3. Without the use of biasing, the remanent induction values left in a previously neutral magnetic recording medium are distorted when compared to the signal that was applied to the recording head.

the recording process, the medium is magnetically saturated, a process which incidentally erases any previously recorded material. During recording, a d.-c. magnetizing force of opposite direction to the saturating field is superimposed upon the signal in such a manner that the combined magnetizing forces produce an induction which, depending upon the strength of the signal, falls between the extreme points A and B of either one of the straight portions of the hysteresis curve. When the different elements of the recording medium leave the recording head, a remanent induction is left in the recording medium which corresponds to the varying induction values of the straight portion of the hysteresis loop but differs from them to the extent that now the magnetizing force is removed and, therefore, a change of magnetization takes place, which is given for the two points A and B by points A' and B', respectively.

This presentation of the d.-c. biasing process is greatly simplified and makes no allowance for demagnetization, which in any practical case has to be taken into account. Demagnetization is a reduction of the retained magnetism, and the degree of demagnetization is a function of the rate of change of the impressed magnetization along the sound track. When the effect of demagnetization is considered, it is found that only one half the straight portion of the hysteresis loop (A–C in Fig. 4) can be used.

In a.-c. biasing, a magnetically neutral material is subjected to the simultaneous action of two fields, one being a high-frequency field of constant amplitude, the other being proportional to the instantaneous signal strength. If again a simplification of the rather complex process of a.-c. biasing is permissible, the field produced by the high-frequency current may be considered as supplying the energy to collapse the hysteresis loop otherwise associated with the signal field, thus bringing about a linear relationship between the magnetizing field of the signal and the remanent induction left in the medium. The picture of a collapsing hysteresis loop implies that each incremental element of the recording medium is subjected to a series of cycles from the high-frequency source while the element passes through the effective magnetizing zone of the recording head gap. Diagrams have been plotted to establish the remanent magnetization in the recording medium as a function of the instantaneous values of the signal strength, taking the complex BH relationship during the magnetizing process into account. These graphical illustrations confirm the experimental evidence that there is a good proportionality between the strength of the signal at any instant and the remanent induction left in the recording medium. To prepare a recording medium for a.-c. biasing, it is necessary that the erasing process return the medium to its neutral condition.

It has been previously stated that the shortest wave length which can be impressed in recording is not only dependent upon the effective gap length of the recording head but is also determined by the biasing process. In accordance with theory and practice, the effect of the gap length in a.-c. biasing is much less important than in d.-c. biasing as long as the field intensity beyond the gap of the recording head decreases rapidly.

As in d.-c. biasing, demagnetization must be considered in a.-c. biasing, but in the latter case the range of linearity is not restricted to half the magnetization curve as it is in d.-c. biasing.

A review of magnetic recording would not be complete without a discussion of one of its most unique applications. Magnetic recording, like any other method of recording, can and has been widely used for temporary and permanent storage of audio signals. But magnetic recording, in its broader aspects, is not limited to this restricted field. For use in a general memory device—for example, in an electrical computer—the principle of magnetic recording has particular appeal because of the high speed with which the signal can be impressed on, and, at will, removed from, the recording medium.

During recent years, the development of suitable recording mediums in particular has provided the practical means for fast storing and quickly locating recorded data. In the early years, subsequent to Poulsen's invention of magnetic recording in 1899, it was the general concept that steel wire and steel tape were the appropriate recording mediums. In fact, steel wire and, to a minor degree, steel tape are still used in sound-recording equipment. In 1929, Pfleumer in Germany produced a magnetically coated paper tape. In coated tape, small particles of permanent magnetic material are imbedded in a binder and serve to provide the required magnetic retentivity.

Pfleumer's idea started a great many development activities—first in Germany, and, during the second world war and thereafter, in this country. The search, which was mostly directed toward obtaining more suitable sound-recording mediums, was extraordinarily successful. The signal-to-noise ratio, heretofore always limited by the surface roughness and lack of magnetic uniformity of steel wires and tapes, has been so improved, par-

ticularly by the use of plastic base materials, that it can be made to exceed 60 db, a value generally considered adequate for high-fidelity sound reproduction. Coated recording mediums, whether paper, plastic, or other materials are employed as base, can be given a configuration which inherently provides the desirable characteristic of a rapidly acting memory device. The plating of magnetic alloys on a nonmagnetic metal surface, a process also developed during the war, has proved equally useful for such applications. It has only to be kept in mind that the recording medium of a memory instrument must expose to the reproducing and playback heads a substantial surface area so that a great many data can be recorded and located with a minimum of time.

The potentialities of magnetic recording for a memory device can best be illustrated with reference to a typical example. Let us assume that the surface of a rotating cylinder is chosen to store one million items. Let us further assume that the existence or absence of a pulse at any one designated

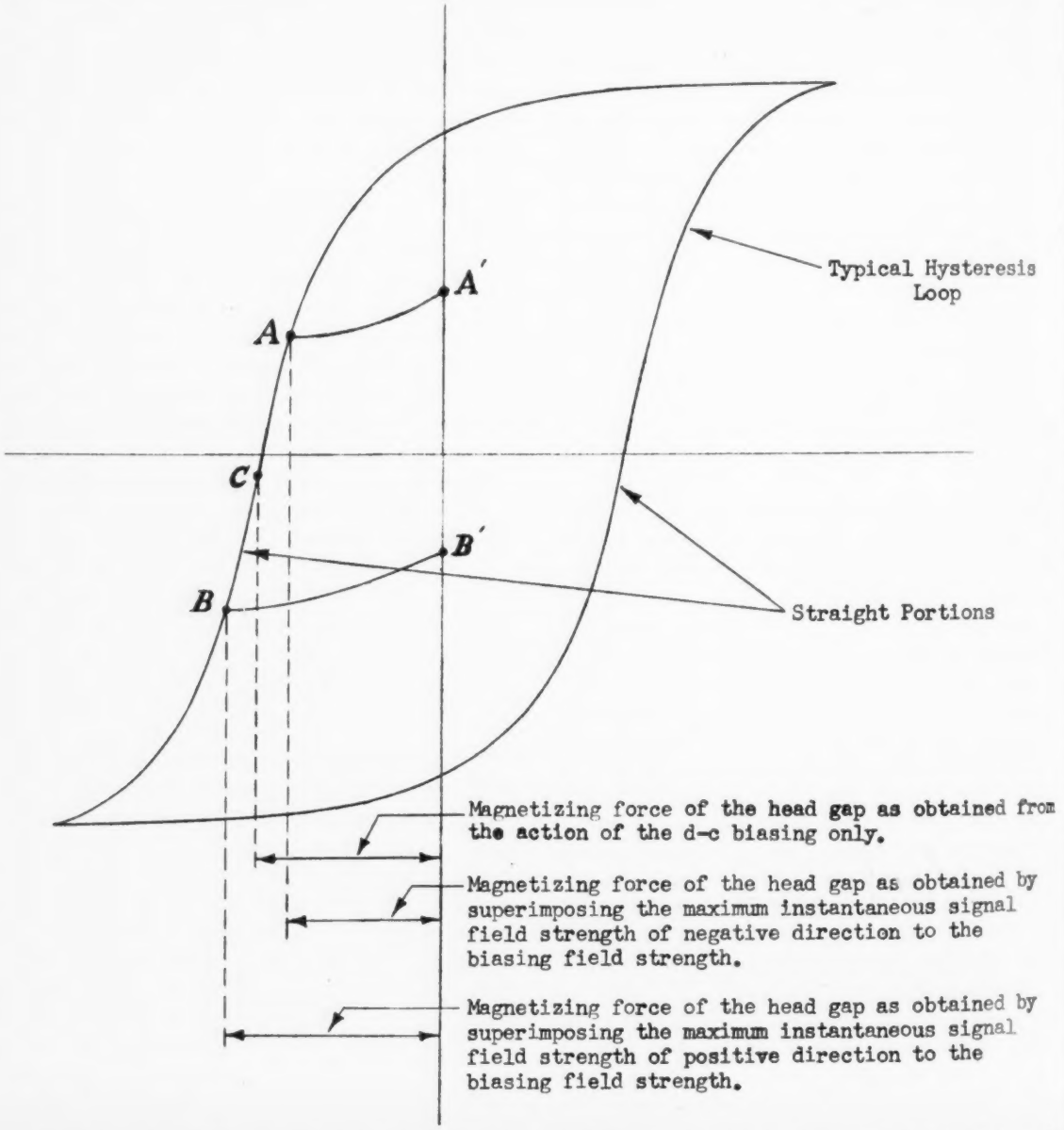
Typical Hysteresis Loop
A'
A
C
B'
B
Straight Portions
Magnetizing force of the head gap as obtained from the action of the d-c biasing only.
Magnetizing force of the head gap as obtained by superimposing the maximum instantaneous signal field strength of negative direction to the biasing field strength.
Magnetizing force of the head gap as obtained by superimposing the maximum instantaneous signal field strength of positive direction to the biasing field strength.

FIG. 4. In d.-c. biasing, elements of a previously saturated magnetic recording medium are subjected to the combined magnetizing force of the signal and d.-c. biasing field, and have remanent induction values along the sound track which are essentially linearly related to the instantaneous values of the signal.

point of the surface provides the yes-or-no information with regard to the one million items. Under these conditions the storage area can be easily calculated. Since each pulse should be impressed or removed without interference of any adjacent one, it is necessary to provide sufficient distance between them to assure trouble-free operation. By spacing the centers of individual pulses 0.005 inch apart along the sound track, adequate separation is obtained. This means that 200 items can be recorded for each inch of sound-track length. If a width of 0.01 inch is allowed for the sound track, and if the sound tracks are spaced 0.005 inch apart, about 66 sound tracks can be provided side by side on a recording medium one inch wide. A set of approximately 13,000 items can therefore be stored on one square inch, and a drum with a surface area of approximately 78 square inches will be able to handle one million information items. This surface area is obtained with a cylinder 5 inches in diameter and only 5 inches long.

Should there be a recording and reproducing head associated with each sound track, the maximum time required to locate any information would be that needed for the cylinder to complete one revolution. The rotational speed of the cylinder is limited only by the mechanical design. Magnetic recording itself does not, for all practical purposes, impose any limitations.

The electronic and switching equipment required to make full use of a memory device of this kind is complex by the very nature of a computer and need not be treated here. But one point is worth mentioning. In a system with only yes-or-no information, no attention has to be given to linearity between the signal-magnetizing force of the recording head and the remanent induction left in the recording medium. Under these conditions no biasing is required. If the amplitude of the recorded pulse is to be used to provide more than a yes-or-no answer, a.-c. or d.-c. biasing becomes a necessity.

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SCIENTIFIC PROCESSES FOR THE IMPROVEMENT OF FARM ANIMALS

IT IS estimated that in 1948 the total national income from the sale of livestock and livestock products was about 17 billion dollars. These products, when processed, amounted to about 24 billion dollars. Of even greater value is the benefit to the population of having at least an adequate supply of animal products for food and clothing.

Farm animals as we know them today have come a long way from the wild boar, wild sheep, and wild ox of prehistoric times. Changes in these animals were a part of the general process of evolution, occurring in response to environmental changes and to the directive action of man. The latter is particularly true for advances made during the past three hundred years or so. Man's interference with nature has come about so gradually that it would be easy to disagree as to the exact length of time that he has taken a major part in directing these changes. Regardless of whether it is three hundred, two hundred, or four hundred years, the changes have been slow, although they have been both progressive and constructive from man's viewpoint.

The first change man probably made in the environment of animals was to begin herding them together rather than hunting them. Subsequently, he must have taken some protective measures. During the hunting period, the wildest and fleetest animals were the ones most likely to survive and propagate their kind. With herding, and the provision of some protection against both wild animals and starvation, a somewhat more docile, and perhaps larger, type became favored, and gradually changes brought about by natural selection began to occur.

As man moved about in his search for a more suitable environment, and as a consequence of his wars with other tribes, he carried his animals with him. This led naturally to the crossing of types. Crossing was followed by breeding from "within," and that brought about segregation, with further opportunity for the selection of types better suited to the environment. Later, man introduced new types into his own stock with the object of improving it, and he also began selecting toward definite objectives. Henry VIII used these methods to improve the race horses of his day. He also promoted the race track, which provided a proving ground for his horses.

During the eighteenth century leading farmers of England and Western Europe settled down to the real business of breeding more useful farm animals, and the rate of improvement was increased. They deliberately searched for superior stocks to introduce into their own herds; they then bred from within their own herds and carefully selected progeny toward definite ends. More or less unwittingly, they used the only tools we have

A Minnesota No. 1 gilt. This type of pig was produced in this new breed in ten years from the time of the first cross.

today for livestock improvement—crossbreeding, inbreeding, and selection. It is noteworthy that several of the English breeders used inbreeding very deliberately at a time when the best biologists of the day condemned the practice. Fundamental biological knowledge had not yet advanced far enough for biologists or breeders to know what was involved in either crossing or inbreeding. The breeders, however, had discovered that they achieved certain objectives by the use of these methods. We must admit that they did a remarkable job when we consider that they violated the edicts of the leading scientists of the time and that there was little past experience to guide them. At the same time, it should be remembered that, for the most part, it took them fifty to one hundred years to make a breed, and that their breeds, when finished, did not possess a high degree of purity.

It should also be recognized that not all the breeds that are prominent today were developed by the orderly process outlined. Several came into being by a hit-or-miss series of events, mostly the result of a community endeavor to produce more useful market animals. If, however, we analyze the methods used we find that crossbreeding, inbreeding, and selection were always used, though with little precision or understanding.

Gregor Mendel, who laid the foundations for that branch of biology known as genetics, at the same time laid a foundation for improved and scientific methods of breeding for both plants and animals. In the period immediately following the rediscovery of Mendel's laws, too much was expected of them by many plant and animal breeders. In general, plant breeders did a better job than animal breeders of continuing to try to adapt Mendel's laws to their work of improvement. The result was that the plant breeder ushered in a whole series of new and modified techniques for plant improvement, techniques which embodied the three fundamental basic principles: crossbreeding, inbreeding, and selection. Plant breeders during this century have made many contributions to society, but the development of hybrid corn was perhaps of most economic importance, and it caught the public fancy. My own work, and that of my contemporaries, in making new breeds stemmed from the successes of the plant breeder, particularly his success with hybrid corn.

The fundamental task of the animal and plant breeder alike is to regroup the genes already in existence so that new gene complexes are produced, which in turn produce more favorable phenotypes. This is largely ignoring mutations and chromosome aberrations, which science has not yet brought under control to any practical extent. Since every mating in farm animals involves several thousand genes, which are grouped on about 40–60 chromosomes, the opportunities for regrouping the genes are almost limitless. Our comprehension of the application of genetics to practical animal breeding is very different today from the ideas that prevailed after the rediscovery of Mendel's laws. At that time the concept was of a small number of genes and of each gene, or at least most genes, being solely responsible for a given end result. Today it is estimated that the number of genes involved in producing a zygote runs into several thousand and that most character expressions are the result of the interaction of many genes. Work today has therefore come to focus more and more on gene complexes in animal breeding.

Crossbreeding is used in constructive breeding for two entirely different purposes: One is to introduce new genes into a population suspected of being deficient in some one or several respects; the other is to induce hybrid vigor through pairing of unlike genes for the production of commercial livestock.

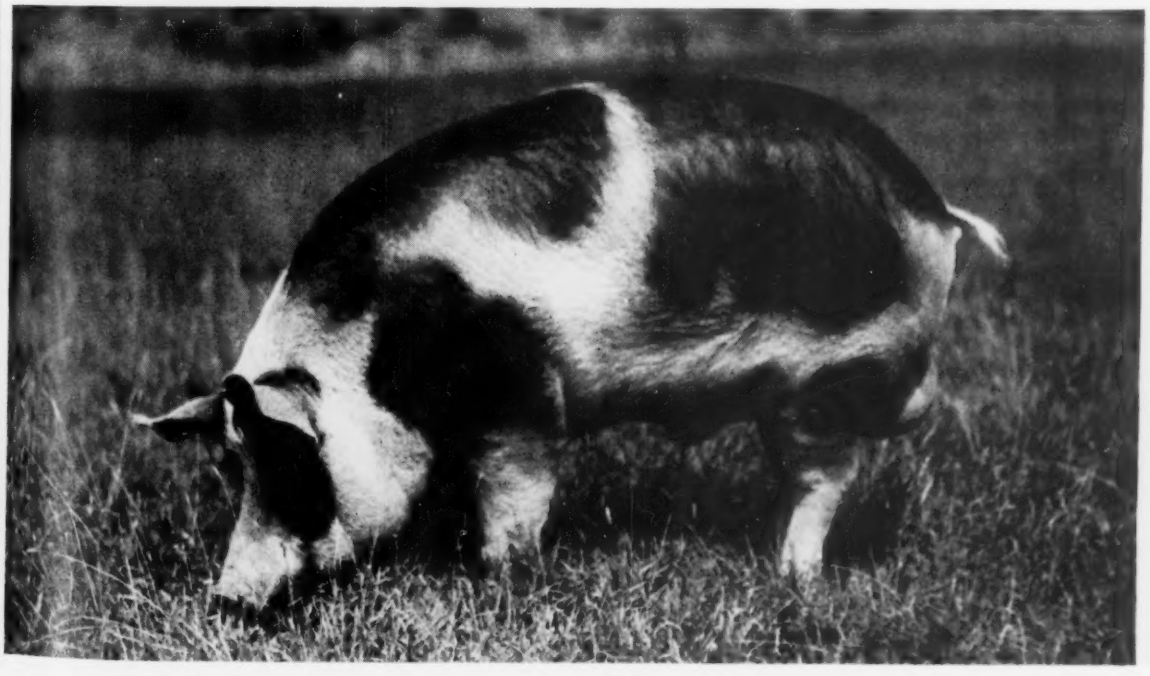
For the most part the older breeds yield some hybrid vigor on crossing. The amount varies between breeds and species; breeds of swine give an increased performance of about 6–7 percent for

a single cross and about 12 percent for a three-way cross. The breeds of sheep when crossed appear to yield more hybrid vigor; it is estimated that it will run from 12 to 20 percent. The Bureau of Dairy Industry, USDA, has released data on the crossing of dairy cattle which show an increased production of from 25 to 32 percent for the crossbreds. The beef breeds of cattle also cross to advantage. The evidence from crossing shows clearly that even though the process by which the older breeds were developed was slow and indefinite, yet considerable separation of genetic material did occur; otherwise there would be no increased vigor from crossing.

Since the existing breeds did cross to advantage, the obvious next step was to develop breeds or lines that would cross to greater advantage. In 1937 a concentrated attack on this problem in swine was initiated through the establishment of the Regional Swine Breeding Laboratory. This is a Federal institution that functions through cooperation with a number of state agricultural experiment stations. The modes of attack on the problem were somewhat varied. All, however, were directed toward the development of inbred lines that would cross to advantage, and all were planned to utilize in some way the three basic tools.

Inbred lines were started from within the existent breeds and from crossbred foundations. Superior lines have been developed from both types of foundations, but the evidence favors those started from the crossbred foundation. This is quite logical because, within reason, the broader the genetic base the greater the opportunity for effective selection after the process of segregation has been started by the interbreeding of the crossbred population. Three new worth-while breeds of swine have been developed during the past few years from crossbred populations; they are designated as Montana No. 1, Minnesota No. 1, and Minnesota No. 2. The Montana No. 1 was developed by the USDA in cooperation with the Montana State Agricultural Experiment Station, twelve years being taken to bring the animals to the point where they were recognized as a new breed. The Minnesota No. 1 and No. 2 were released ten and seven years, respectively, after they were started. This is in contrast to the fifty to one hundred years required to develop the older breeds. Furthermore, as measured by the coefficient of breeding, these new strains carry about three times the amount of genetic purification of the older breeds.

In a sense these new breeds are "tailor-made." This does not imply that everything desired has been bred in them; it does mean, however, that they were developed according to plan and that at least certain definite qualities were bred in them. What is more, the Minnesota No. 1 and No. 2 were planned to make a superior crossing combination,



A Minnesota No. 2 boar. This breed was made in seven years by the application of modern scientific principles.

and they do cross well. The crosses are yielding a strong 20 percent superiority in performance. Carcass quality is one of the factors included in the appraisal of performance; the carcasses of these two breeds yield an 18 percent reduction in fat over standard breeds reared under similar conditions.

The development of new breeds of inbred stocks to be used in crosses for market animal production offers the professional animal breeder an almost unlimited opportunity for the improvement of our farm animals. The whole principle rests on the sorting and regrouping of the genes. The process rests on the science of genetics and, finally, on utilizing the fundamentals of hybrid vigor.

Modern selection embodies the basic principles of statistics. In order to use statistics satisfactorily, it was first necessary to develop adequate techniques of measurement; the performance factors of farm animals are not measured by eye appraisal. The development of satisfactory techniques for measuring performance is still in the embryonic stage, but enough has been accomplished to indicate plainly that more progress can be expected through further refinement of measurements. A large portion of the refinement must come through a greater control of environmental influences. It is estimated that for most economic characters used as a basis for selection about 60–80 percent of the variance is due to such influences. In other words, the breeder at present cannot hope to advance the genetics of a population more than 20–40 percent of the selection differential.

Scientific processes have invaded the field of animal improvement. They involve a knowledge of basic genetics; for the most part the identity and manipulation of individual genes plays but little part in this process, since quantitative characters are generally dependent upon the interaction of many genes. An understanding of heterosis and an appreciation of its applications are fundamental. Inbreeding, crossbreeding, and selection are the tools for shifting genes into different combinations. Proper selection involves at least a partial control of environment, the development of more precise methods of measurement, and the utilization of statistics.

What has been done is barely a beginning. The work can be speeded up, and animals can and will be developed that are far better suited to specific purposes and to our ever-changing demands.

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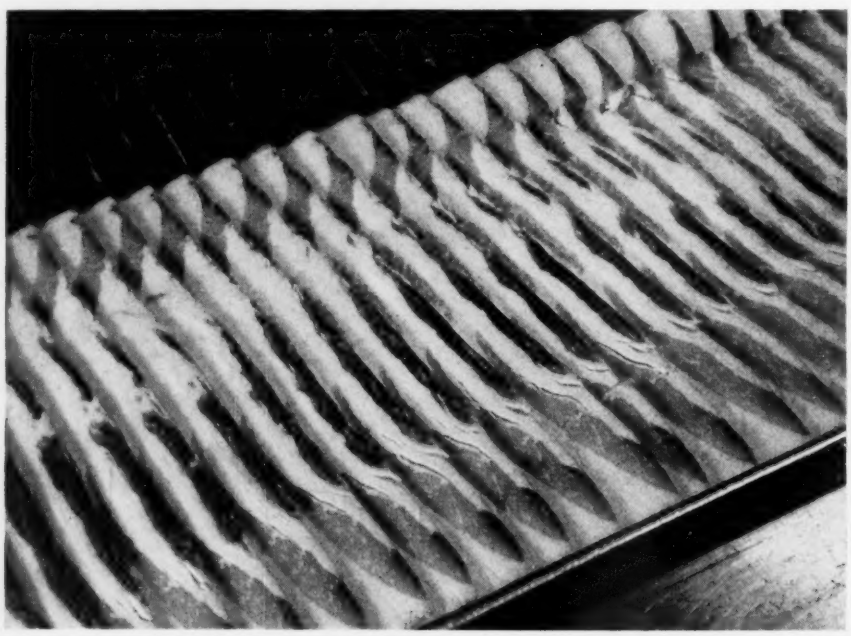
Type of bacon produced from a cross of the Minnesota No. 1 and No. 2. Note the high percentage of lean meat.



BOOK REVIEWS

UP FROM THE APE

A New Theory of Evolution. Sir Arthur Keith. x+451 pp. $4.75. Philosophical Library. New York.

IN THE sixty years since he began his comparative studies of monkeys and apes, Sir Arthur Keith, the *doyen* of physical anthropology, has added much to the literature on human origins. His previous studies have been mainly concerned with possible stages in physical evolution, and he has not shown, at any rate in his writings, much interest in what might be called the living context of physical change. Three years ago, however, he published *Essays on Evolution,* a volume in which, among other themes, he elaborated the view that nations, although political units, should also be regarded biologically as evolutionary units. His new book, we are told in the preface, is an exposition of the fundamentals on which this earlier volume of *Essays* is based. It deals with Sir Arthur's new "theory of man's evolution," with the demarcation of mankind into its major divisions or varieties, with the role played by "race" in evolution, and with the rise of nations.

Like those that have gone before it, the book is eminently readable. Its essential thesis is that human evolution has taken place by the physical transformation of individuals within small social groups, first of apes and then of proto-men, and that larger social groupings have been derived from these smaller groups. All this is readily acceptable, and indeed there is nothing very novel about Sir Arthur's proposals. Evolution is not something that occurs among the skulls on the shelves of a museum. It has always shown itself by the emergence of new physical and behavioral characteristics within the context of a social life. Scientific readers will, however, find that Sir Arthur's re-emphasis of this fact is associated with much strange and special pleading.

The book begins with a consideration of the social organization of primitive man and of the subhuman primates, and Sir Arthur speculates convincingly about the factors which hold small groups together—about such matters as territorial associations, kinship, patriotism, cooperation, and so on. "A group to survive," he writes, "must have amity and unity at home, and a will to resist attack from without." Isolation in groups, according to Sir Arthur, is essential for evolution. But obviously it is not the whole story, for otherwise, he asks, why have the apes, with the same group organization as primitive man, failed to evolve? This question is answered in the middle section of the book. Some group or groups of anthropoids became possessed of genes which led to an upright posture, and so man began his march toward humanity. The particular proto-men who embarked on this adventure were the African fossil anthropoids, and here Sir Arthur reverses his earlier and scientific caution, and swallows the speculations of Drs. Broom and Dart in their entirety. The African ape men spread, and gave rise to the five main divisions of mankind, whose diversification into races and national groups—biological units of evolution—takes up the final third or so of the volume.

It would be idle to underline the many shortcomings in Sir Arthur's treatment of the well-known doctrine of evolution in small groups, or in his discussions of the interaction of gene changes and selection in evolution. It is also unnecessary to comment on the many tautologous statements and contradictions—for example, about the force of "human nature" which on one page "takes the place of instincts," and on another is itself possessed of "instinctive actions." But it is necessary to show why the book fails as a serious scientific contribution. The reason is fairly clear. In spite of its title, Sir Arthur's new book completely begs the question of the problem of human evolution. He asks, for example, what were the mental gifts that gave ape men evolutionary ascendancy in the world of living things? And he answers, quite simply, the same qualities of leadership as characterize such modern human leaders as Stalin and Archbishop Temple. What, he asks, is the mechanism which underlies the evolution of the specific characters of the human body? And again the answer is there—changes in the system of hormones. What, again, was the first essential change that transformed an ape into man? Once more we have a ready solution—the sudden possession of genes which assured an upright posture. And, last, how difficult to the anatomist is the problem of the transformation of an ape to a man? To Sir Arthur, not difficult at all. "Such a transformation implies merely an increase in the organization and in the size of the brain, with a reduction in strength of jaw and teeth."

It is that "merely" which makes the new "theory" less a serious work of science than an example of how words can be used to by-pass problems. Writing this is hard for a reviewer who has learned much from Sir Arthur's earlier works. But it needs to be said. Physical anthropology is at present sick from a plethora of writings by a number of its older disciples who find composition and ex-cathedra statement easier than biometric study. Years of hard scientific work may be needed to reverse the setback it has suffered recently from proponents who seem prepared to go to any lengths of special pleading in order to further what Sir Arthur calls "an evolutionary credo."

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PROBING PSYCHODYNAMICS

Psychodiagnosis. Saul Rosenzweig. xii + 380 pp. Illus. $5.00. Grune & Stratton. New York.

ONE of the promising signs of our increasingly complex and specialized age is an awakened self-criticism and a growing insight into some of our own and society's adjustment problems. An era of real individual maturity and enlightenment is still in the future, but a wider acceptance by the public of the need for modification or intensive restructuring of the personality with the help of skilled psychiatric and psychological therapists is evident.

The relatively new field of clinical psychology has emerged to help meet this challenge of reconstructing the sick and troubled personality: and, as a first step in the therapeutic process, these clinicians are being entrusted with the responsibility of charting the dynamic factors underlying the present status and functioning of the patient. In *Psychodiagnosis,* Dr. Rosenzweig, with the help of Dr. Kate L. Kogan, takes the reader into this field of exploring, describing and predicting the key factors underlying human behavior by means of psychological tests.

In succession there are presented fairly brief but instructive descriptions, followed by discussions of major instruments currently in clinical use for measurement and evaluation of intelligence, of intellectual impairment, of vocational aptitudes and interests, and for appraisal of personality both by means of questionnaires and inventories, as well as by the richer projective techniques. The material on each of the instruments is followed by illustrative test protocols which bring into sharper focus the particular diagnostic values and limitations of the tools in real-life situations. Then, after stressing the importance of the "battery approach"—by means of which the clinician seeks to obtain a well-rounded picture of the individual by employing a number of complementary procedures plus additional clues—the author demonstrates how thereby an integrated view of the patient may be obtained. The last half of the book is in essence a series of examples of the fused science and art of the psychological clinician.

Of course it is possible to question the inclusion of some of the materials selected for presentation and the omission of others. For instance, among the projective techniques, the Szondi and the Bender Gestalt tests, on which considerable research material became available sufficiently prior to publication, would seem to warrant attention, along with more ample treatment of the personality implications of human figure drawings. Similarly, graphology, the "action techniques," and the revealing play in "control-situations," "frustration techniques," expressive finger painting, plastic and balloon games, might well have been included within the scope of the book as implements both of diagnosis and of the larger therapeutic process.

Nevertheless, although the really advanced clinical psychologist will gain relatively little in new or refined skills by study of the work, even for him it has value as a convenient compilation, which offers as well good illustrative case material. Most significantly, though, in providing students, younger people in the field, and professionals with a scientific interest in psychodynamics with an up-to-date, readable, compact yet authoritative review of techniques employed by clinical psychologists today to gain a better understanding of the functioning, endowment, interests, drives, conflicts, and problems of their patients, Rosenzweig has made an important contribution.

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A VISIT WITH THE MacGINITIES

Natural History of Marine Animals. George E. MacGinitie and Nettie MacGinitie. ix + 473 pp. $6.00. McGraw-Hill. New York.

THE high light of any trip to the West Coast is a visit with the MacGinities—husband and wife—if you are so fortunate as to know them or go to see them after having heard about them. One is always welcome, and aside from the great pleasure found in their company on such occasions, you learn a lot of real biology, of the lives of the animals that live in, or draw their sustenance from, the sea, their kinds and classification, their habits and their habitats, their behavior and its variation, the relationships they bear to one another and to the world about them, succession in animal communities, what animals do, what they eat, how long they live, the evidences of their existence in life, and also after death—things for the paleontologist as well as the recent zoologist to ponder over. Before the spell of your visit is over, whether it be a matter of minutes, hours, or days, and whether it sounds appropriate or trite, you invariably end up with "You ought to write all this down." They have written it down, much of it, in this book. Moreover, they have dedicated it to the friends and visitors who thought it should be done.

It is couched in simple, easy-to-understand language, a rarity in these days of extreme specialization in science and concomitant specialized vocabularies. Where special terminology had to be employed because of the subject matter, all such terms are carefully defined in appropriate places in the text. The pronunciation and derivation of the animal group names are included along with the scientific names of the animals, so necessary for further reference to them in literature, laboratory, or field. By these means, the authors, both highly regarded ecologists, have achieved the happy result of writing informatively for the layman and the student without offending the more catholic tastes of the professor or the scientific authority, and so give this impressive journal of personal experience and observation the wide appeal it justly merits.

It is a marvel that two people working together

could have seen and done, read and written, so much, have done it so well, and embroidered it so entertainingly with factual animal anecdote and other pertinent comment on the ways of animals, and on man, and how at least some biological work should be conducted and what it takes to do it.

Among the animal-interest stories there is the one of their "pet" hermit crab. Through the shell a small hole was drilled at about the place where the crab fastened on with the tip of its abdomen. A little prodding through this aperture would cause the crab to drop out of its shell immediately. The MacGinities used to pick this crab up and prod it out of its shell for the entertainment of visitors. So often was this performance repeated that finally the crab would drop out of its shell whenever it was lifted out of the water. It would then rest on the bottom of the aquarium until its shell was dropped, whereupon it would quickly grasp the shell and re-enter it.

Where the field is so large it is not to be expected that all scientific names will be in accordance with the lastest findings of the systematists; or that all the pertinent literature could be satisfactorily or adequately covered. For example, the name long applied to one of the commoner California pistol-crabs—more properly a shrimp, *Crangon dentipes* (p. 277, Figure 128)—has been replaced by the very appropriate one under which it was originally described, *C. clamator*. In view of the reliability of the MacGinities' own personal observations, it is to be regretted that it was not possible for them to check or verify the material gleaned by them from the published work of others. In a few instances, bits of misinformation have crept into their otherwise excellent text.

In their reference to the tree-climbing coconut crab of the Indo-Pacific (p. 301), which feeds on other and smaller palm fruits often so obtained, the MacGinities unwittingly repeat the long-popular idea that this crab is able by its own unaided efforts to open ripe coconuts. The reviewer himself feels in a measure responsible for the perpetuation of this assumption handed down by a long series of authors, most of whom copied earlier, unverified statements or merely reproduced hearsay. Within the past decade, another author, in a carefully documented survey of earlier literature, called attention to the fact that no naturalist has yet witnessed a coconut crab opening an undamaged nut, and that, so far, all feeding experiments with mature whole coconuts have proved unsuccessful. More recent experiments repeated on Christmas Island in the Indian Ocean likewise failed.

The many half tones that illustrate this natural history may have necessitated the high-gloss paper used throughout, but it is neither well adapted to marginal notes, which many a reader will be intrigued to make, nor suitable for use in laboratories —where it undoubtedly will be in constant demand— for the inevitable wetting will damage the pages, if it does not cause them to stick together. The generally unique and intimate photographs of marine habitats and animals deserved better reproduction than the publisher seems to have been able to render.

Altogether too often has it happened that a real naturalist, in every sense of the word, found the observing, the experimenting, and the telling far more fascinating than the recording and writing. Virtually everyone can recall at least one fine naturalist of this sort, who possessed a wealth of firsthand information regarding marine animals but got little or none of it into print. Happily, the MacGinities have proved an exception to the rule. The general reader as well as the researcher will ever be grateful to them for writing down so much of what they learned in so inspiring a book, suggestive as it is of a thousand theses and biological investigations; to the publisher for having undertaken this fine effort for the MacGinities; to Thomas Hunt Morgan for setting them up in Cal Tech's Kerckoff Marine Laboratory at Corona del Mar, where so much of their original work was carried on (this reviewer thinks it was the best thing he ever did); and to the professors of their Stanford days at the Hopkins Marine Station at Pacific Grove, notably Harold Heath and Walter K. Fisher, for having turned out two such well-grounded and accomplished students as Nettie and George MacGinitie. The latter, by the way, is the newly appointed scientific director of the Arctic Research Laboratory at Point Barrow, where Nettie will serve as his research associate.

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A DIP INTO THE FUTURE

Must We Hide? R. E. Lapp. x + 182 pp. $3.00. Addison-Wesley Press. Cambridge, Mass.

ATOMIC bombs, the way in which they might be used, and the damage which might result are the subjects of *Must We Hide?* These are old subjects (nearly four years!), and most of the facts presented are already available somewhere in the literature, but the contribution of this book lies in the assembly and interpretation of these facts in simple language, and in the deduction from them of some important conclusions.

The immediate and lingering hazards of radiation, the vulnerability of skyscraper cities, the possible methods of bomb delivery and of defense against them are analyzed in a deliberately cool manner. A variety of background data is offered, including some interesting detail from the U. S. Strategic Bombing Survey.

Dr. Lapp is a well-trained physicist who happens also to be well informed about atomic bombs. He has had an extensive association with atomic energy matters, first in the Manhattan Engineer District, and later in the National Military Establishment. His serious consideration of unpleasant facts is therefore a matter of some importance. He is to be commended for avoiding sensationalism, but the reader will be

a little disappointed to find that the author's writing style is not sufficiently buoyant to prevent the book from becoming a bit of a chore instead of a pleasure.

The most interesting part is probably the author's plea for the dispersal of great cities. He insists on its desirability apart from the possibility of further war, and that this possibility simply clinches the argument. His methods for achieving such a large objective may or may not seem feasible to the reader, but his analysis of the problem deserves attention and his challenge to face the issue must be accepted.

In choosing a title, Dr. Lapp obviously played upon the words of Dr. Bradley's *No Place to Hide*, but he gave the words a different meaning. Whereas the earlier title can be interpreted literally in a physical sense, the theme of this book is that "We must not hide from the facts." It seems likely that Dr. Lapp's reason for this choice of title was to emphasize his commendable effort to place in broader perspective atomic bomb hazards in general and radiation dangers in particular.

Further efforts to meet the need for a level-headed appraisal of atomic warfare potentials will be welcome. As long as the possibility of another war persists, the problems and the facts discussed in this book must be widely faced and widely understood.

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FOR ANTHROPOLOGISTS AND OTHERS

Most of the World. The Peoples of Africa, Latin America, and the East Today. Ralph Linton, Ed. 917 pp. 19 maps. $5.50. Columbia University Press. New York.

AT THE mid-point of the twentieth (so far mankind's most terrifying) century, anthropology has become one of the significant voices speaking in the wilderness. And, one might also point out, there has been less crying and beating of breasts in accompaniment to its utterances than has been the case with most other professional sages that are on the scene today. The cross-cultural point of view contains a high element of objectivity. Its descriptive-comparative findings are presented dispassionately. The book *Most of the World* follows in this anthropological tradition. No Cassandra-cry, it is not even a call to arms. It is simply a statement of: "Here are the facts. Do with them what you will. But do not forget that in this world there are contemporaries of ours who outnumber us at least 10 to 1 and who have lived their entire lives under motivations and with values that are completely alien to the European-American patterns of behavior that seem so natural to us." The anthropologists, sociologists, and economists who have contributed the essays for this symposium offer no pat solution for "saving the world." They are, however, interested in realities and in the examination and understanding of all aspects of human cultural behavior. They are aware that the emphasis of any one segment of such behavior, if done at the expense of other segments, lessens the reality of even the particular aspect overemphasized. International credit is indeed a very real thing; but those who see the destiny of the Near East or Indonesia only in those terms will eventually become just as remote from the totality of the problem as a high-caste Hindu, whose quite different vision of a sane and well-ordered world also appears to him as indisputably correct.

Like Linton's earlier compilation, *The Science of Man in the World Crisis*, the present collection of essays is an attempt to make available to the non-specialist the findings of anthropology. Each of the twelve articles is written by an outstanding regional or topical specialist: Howard A. Meyerhoff, "Natural Resources in Most of the World;" Stephen W. Reed, "World Population Trends;" John Gillin, "Mestizo America;" Charles Wagley, "Brazil;" H. J. Simons, "Race Relations and Policies in Southern and Eastern Africa;" William R. Bascom, "West and Central Africa;" Carleton S. Coon, "North Africa;" F. L. W. Richardson, Jr., with James Batal, "The Near East;" Daniel and Alice Thorner, "India and Pakistan;" Raymond Kennedy, "Southeast Asia and Indonesia;" Francis L. K. Hsu, "China;" and Douglas G. Haring, "Japan and the Japanese, 1868–1945."

A critical analysis of all these papers would require several pages and is, moreover, beyond the experience or capabilities of the reviewer. Each writer is steeped in his own material and possesses a great wealth of detail as well as insight into his subject. All articles are well organized and written. Meyerhoff and Reed have offered information, respectively, on the topics of natural resources and world population. Both Coon and Kennedy give fascinating accounts of Colonial policy and history. Hsu, the only non-Western author, combines an "insider's" knowledge of culture with modern anthropological and sociological training and techniques. In brief, each areal contribution is a "vest-pocket" monograph covering both overt and covert culture in the territory selected.

Will this book and others like it be used by those who are guiding our social, political, and economic destinies? A review of past history does not offer much hope on this score, but on the other hand cultural objectivity is a recent concept. We can only wait and see. Linton, in his excellent introduction, speaking of these vast areas and peoples of the world with whom we must work out together some design for living, closes with:

> It is hard for Americans, reared in the traditions of European ethnocentrism, to appreciate the importance of these emergent powers. In population and natural resources they represent most of the world and they are moving toward technological equality with the West at a startling rate. The purpose of this book is to give an accurate picture of the conditions which exist in most of

the world today in the hope that this may assist in the formation of public opinion and may provide a basis of sound knowledge for future planning. The task which confronts us now is that of trying to reconstitute one world on the basis of collaboration rather than domination. We must devise techniques to conserve the advantages of the former European hegemony as far as possible while rectifying its injustices. Any realist must recognize that the chances of accomplishing this are far from good at present. However, we must find what consolation we can in the knowledge that if we fail there will be others to try again, and again, until world unity is achieved.

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ALL KINDS OF DATA

Human Behavior and the Principle of Least Effort. George Kingsley Zipf. x + 573 pp. $6.50. Addison-Wesley Press. Cambridge, Mass.

IF WE consider the population of the major cities in the United States, from the largest to the smallest, we notice that the second largest city is only about half the size of the largest, the third largest is only about a third the size of the largest, etc. This holds true not only at the present time, but throughout the whole history of the United States. In addition, this relationship holds true for practically all other major countries. We may also go to a vastly different area of human behavior, the use of language, and find that a similar type of relationship holds. If we compare the most frequently used word with the next most frequently used word, we discover that the second is used only about half as often as the first, and further that the third is used only about one third as often as the first, and so on. If we plot a frequency distribution of data of this sort, with the base line ordered according to rank frequency, we obtain a descending function which approximates very closely a hyperbolic curve. Or, if we plot this hyperbolic curve double-logarithmically, we obtain a straight line of negative slope. Scientists have long been familiar with the near universality of the Gaussian curve, in which the base line represents some quantitative variable, and the ordinate the frequency of occurrence. It appears, however, that those kinds of data that cannot be ordered except in rank order of size have not been studied extensively. When frequency distributions of this sort are made, it turns out that there are many, many areas of human behavior in which this type of distribution approximates a simple hyperbolic function. The best examples are the two given above.

This book consists of a vast array of such data drawn from such varied fields as linguistics, psychology, economics, sociology, population, music, politics, and even taxonomy. Most of the examples are ordered in such a way that the frequency distributions approximate straight-line functions when plotted double-logarithmically. The amount of work involved in collecting all this varied data was prodigious.

The main thesis of the book is that the regularity of this material may be explained, in large part, by the principle of least effort: "An organism will expend the *least average probable rate of work* (as estimated by itself)."

The first third of the book deals with linguistic data, with examples of language samples drawn from practically the whole range of human language: American newspapers, English novels, primitive languages, Chinese, Gothic, Old English, Old High German, vocabulary of a young girl at different ages, and samples of schizophrenic speech. Most of this material is presented graphically, and the empirical data fitted to straight lines. The ordered regularity of such a vast amount of varied material is surprising. The middle several chapters of the book deal with language as a psychological process, with the origin of the "ego," with the economy of the symbolic process, and with other psychological processes, including dreams. In practically all cases, these processes are viewed as instances of the principle of least effort. The last several chapters of the book deal with social phenomena, all of this material viewed as "a case of intraspecies balance." Here is presented a large number of hyperbolic functions, including size of metropolitan districts, manufacturing establishments, retail stores, occupations, movement of freight, distances of long-distance telephone messages, distances separating applicants for marriage licenses, incomes, and many others. This book, if for no other reason than the sheer array of data presented, represents a signal contribution to the behavior sciences.

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STUDY OF THE ORIGIN OF CANCER

Cancer Radiations, Virus, Environment. Vol. II. J. Maisin. 306 pp. 120 fr. Casterman, Tournai. Paris.

IN THE periodical *Science* (1949, **109**, 21), we commented on the first volume of this book. There, Maisin covered the extensive fields of heredity, hormones, and carcinogenic substances; in this second volume, the Belgian worker discusses the equally large, or still larger, fields of radiation, viruses, and environment. Now that the whole book is finished, one cannot help but feel a genuine admiration for the task accomplished of giving a coherent expression to the almost chaotic knowledge we have of cancer.

With increasing frequency in our times, books are written on various subjects by many persons and edited either anonymously or by one person with a well-known name. In either case the reader is given just the factual information or, at best, some unilateral views on the problems, and from these data—in most cases conflicting—and from an inhibited discussion—generally a mere formality—he is supposed to draw

his own conclusions. This procedure may be acceptable if the conception one has of a book is that of a more or less useful compilation of facts put together according to some pattern; but it is quite inadequate if one thinks of a scientific book as a conceptual endeavor, in which each concept is the synthesis of many facts.

The latter is what Maisin has done with his monograph, which is why the book is not only useful but enlightening. The sensation of unity the reader experiences is that conveyed only by those who are familiar with all the facts of a complex biological problem, both in the clinic and in the laboratory. If, on the one hand, Maisin's book reflects a considerable effort, on the other it gives that specific, indefinable impression of ease only achieved by the few who in their maturity can write about what has obsessed them for a very long time.

Precisely because of its unity, the book cannot be analyzed separately in its different sections, since these sections really complement one another. We should like to single out, however, the section on viruses, in which these agents as factors in the genesis of cancer are given their due importance, although one could hardly say that Maisin is a follower of the so-called virus theory of cancer. In the last eight pages of his book he lucidly summarizes the few fundamental concepts at which he has arrived, and on this basis tries to formulate an explanation for the cause of the disease. This he does by attempting to reach a compromise between the indubitable exogenous viral nature of some cancers, and the effects of endogenous inanimate carcinogens. Briefly, the initial carcinogenic effect would be on the nucleus which, in turn, acts on some components of the cytoplasm, which may become autonomous and then behave as viruses.

Maisin is not the only worker belonging to what can be called the "reconciling school," which attempts to find a logical common denominator for the conflicting views on the origin of cancer. Whether the logic of the school is valid or whether it represents an understandable but fruitless effort to reconcile what is scientifically unreconcilable must be left both to the judgment of the reader and to the advent of more discoveries.

We want to end by strongly recommending that this work be translated into English.

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BACTERIOLOGY UPSIDE DOWN

Peace or Pestilence. Theodor Rosebury. 218 pp. $2.75. Whittlesey House. New York.

IT IS of interest to the layman to learn that biological warfare, or "bacteriology upside down," the synthesis of epidemics, will be directed largely against civilians; also that either incapacitation or psychological implications may prove more important than actual killing. There is, however, no background of actual experience. "B.W.," unlike atomic warfare, could be conducted by small nations at low cost and would be hard to control by international authority.

As indicated by the author's long list of sources, there has been much written on the potentialities of biological warfare. Authoritative publications, such as the official statement of Mr. Forrestal and the earlier Merck report, have provided sufficient sound background for those working in fields of bacteriology, epidemiology, and the like. The foregoing official and other unofficial sources have regrettably provided much for exploitation by sensationalists, who are inclined to employ a technical discussion as a springboard from which to leap into political and philosophic considerations.

Such portions of *Peace or Pestilence* as deal with bacteriology and the appreciation of biological warfare are technically sound and deviate in no significant respect from the factual content of official publications. The political and philosophic chapters 11 and 13 merely emphasize the point made by the author on page 155 in speaking of scientists working during the last war: "Necessity lifted them out of their own familiar laboratories and transplanted them into quite unfamiliar soil."

Dr. Rosebury may believe that the only defense against weapons is the establishment of a permanent peace. To assume that such is likely to be accomplished or to be brought about by emphasis on the horrors and possibilities of lethal agents is to ignore the pattern of history.

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THE FAMILY CULICIDAE

The Natural History of Mosquitoes. Marston Bates. xv + 379 pp. Illus. $5.00. Macmillan. New York.

THE introduction to this book offers three major reasons why an animal group receives intensive study. These are: The economic importance of the group, the ease with which its species may be manipulated in the laboratory, and its attractiveness to collectors. In all, the family Culicidae ranks high, and the result has been the accumulation of a vast quantity of facts about mosquitoes: as living organisms in nature, inhabiting its quiet waters, plaguing man and animals, and bearing many serious diseases; as laboratory subjects for students of insect physiology, behavior, development, and disease transmission; and as a fascinating occupation for those whose greatest joy is to note and classify the great diversity exhibited by a group whose basic pattern is notably uniform.

Because of this, mosquitoes are one of the leading families of insects from the standpoint of hours devoted to their study, but in spite of it, a great deal of the knowledge is scattered and unorganized. Whenever the facts on any phase of the work are gathered

together, many gaps are found. Dr. Bates has brought together the salient facts on all phases of mosquito study dealing with the living organism, both in the field and in the laboratory. The 48 pages of bibliography attest to the quantity of literature that was studied to compile the work. It is far more than a compilation, however, since it embodies the important ideas of the author on numerous fundamental problems of animal biology and evolution. He has had many years of close contact with mosquitoes, from the marshes of Albania to the treetops of Colombian jungles, supplemented by long hours of study in the laboratory.

This reviewer can find no aspect of the relationship of the living mosquito to its environment, as exemplified by different species and groups, that is not well covered. As a taxonomist, he is particularly pleased with the treatment of mosquito classification and speciation, with its emphasis on the importance of nonmorphological characters for specific differentiation. The scope of the book is so wide that any zoologist would find it profitable reading, and this includes the physiologist, behaviorist, and the medical man dealing with insect-borne diseases. Dr. Bates has shown ably what we now know about mosquitoes and the relation of this knowledge to biology as a whole, and he points out clearly the most profitable paths for further research in the field.

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CONJURING UP TOMORROW

Nineteen Eighty-Four. George Orwell. 314 pp. $3.00. Harcourt, Brace. New York.

IT IS evident now that *Animal Farm* was a trial kite in political science fiction, sent up to determine the direction of the wind for this, the major flight. The former novel was satirical humor. This one dispenses with humor. The former novel seemed to stem only from the Soviet system. This one adds, to the clearly Russian impact, a picture of the ultimate end of English socialism ("Ingsoc") as it appears to be tending. It is magnificent and deadly satire, humorous only as is the bitter, sardonic laughter of man's utmost extremity. According to the *Saturday Review of Literature*, it "is the most compelling novel to appear this year." Critics on every book-review page hail it as a mighty "what-can-be" and "fantastic-horror" novel.

The story is of an average man, Winston Smith, who lives in London, the "chief city of Airstrip One, the third most populous of the provinces of Oceania," only thirty-five years hence. On the wall of his room is a "telescreen." Before his window hovers a helicopter of the Police Patrol, snooping. He lives with the knowledge that every act is seen, every sound is heard. He is a member of the Party, a worker in the Ministry of Truth, one who rewrites the past so that "Big Brother's" predictions always fit the eventualities. Posters of Big Brother's face—much like Stalin's—are everywhere. Slogans are omnipresent: "War is Peace," "Freedom is Slavery," "Ignorance is Strength."

Other workers are rewriting the language into "Newspeak," to reduce everyone's vocabulary so that only authorized thoughts can be voiced. To "crimethink" is to invite vaporization. Science has ceased to exist except as a study in what "another human being is thinking" or in how "to kill millions of people in a few seconds." War is ever-present; it is a way to limit production, to keep the "proles" underfed, to encourage ignorance, to increase the power of the Inner Party.

For power is the aim, power as an end in itself. Romance, family relationships, and individual personality must be in subjection to it. Early in the book, Winston Smith is a rebel and a seeker for the underground movement called the Brotherhood. Before the end, he—and the reader—knows that there can be no such group, that resistance is impossible. Pages of torture prove it.

It is a frightening book because of its very timeliness. Plainly, the seeds for future horror are being sown now. Totalitarianism is a first symptom, state socialism follows, and after that total destruction of our civilization. Mr. Orwell, "indignant and prophetic," calls upon man to beware, to stop, to look where he is heading. It is an emphatic warning and a challenge.

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BEGINNING OF THE INDUSTRIAL REVOLUTION

James Watt and the History of Steam Power. Ivor B. Hart. vii + 250 pp. Illus. $4.00. Schuman. New York.

THE title of this book has been carefully worded so as to avoid a common misconception of Watt as the inventor of the steam engine. As a matter of fact, the development of steam power was a gradual growth, covering several centuries. The book under review describes this development, not neglecting the scientific research that produced a body of laws and facts of thermodynamics which made possible the design of the steam engine.

The earlier pages of the book give an interesting picture of the dawn of human control over nature; and, coming down to the England and Europe of the time of Watt, the book describes the domestic industrial system then in existence in England, when the weavers and spinners labored under their own roofs and were not yet concentrated in factories.

The first steam engines were designed to act as pumps to remove water from mines and deep wells. The most important predecessors of Watt were Savery and Newcomen. In the pumping engines

designed by these inventors the cylinder acted also as condenser. The time necessary for heating and cooling the cylinder made these engines rather slow-acting. Newcomen's engine of 1712 operated at ten strokes per minute, but even at that rate it was much more efficient than manual labor or animal power.

Watt contributed two important improvements to the design of the steam engine. He introduced a separate condenser, which greatly accelerated the rate of working, and also saved steam and coal necessary for heating the cylinder before each stroke. He also introduced the double-action principle, by which steam pressure acts in both the forward and backward motion of the piston. The steam engine as thus developed by Watt remained unaltered in fundamentals for a century, until the introduction of the steam turbine, about 1884.

Hart's book gives an excellent picture of the whole subject, pointing out that it was Watt's engine that *produced* the factory system by bringing the textile workers from their separate homes to a common workshop. There is also an interesting aftermath which discusses, among other things, the ethical aspects of the industrial revolution. The book as a whole can be recommended to nonprofessional readers.

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USEFUL COMPENDIUM

Animal Encyclopaedia. Mammals. Leo Wender. 266 pp. $4.50. Oxford University Press. New York.

THIS is the kind of book that many of us of the older group of zoologists wish we might have had when we were much younger.

It is an alphabetical arrangement of the vernacular names of mammals, with short comments touching upon the range, number of species, size, classification, weight, anatomy, economic importance, and other pertinent facts. About 1,798 vernacular names are listed, and under most of the vernacular names the generic name and sometimes the specific name are mentioned. The "Latin index" lists the scientific names alphabetically, and opposite each appears the vernacular name; these are arranged alphabetically in the main text. Three pages are devoted to "Gestation Periods and Numbers of Young at a Birth." This lists about 150 names, with the gestation period and number of young for such as the author apparently had available. The question marks at several places in the column marked "gestation" and in the column marked "young" are valuable in that they emphasize the need for obtaining and publishing more information regarding these important phases of the development of animals. Four pages are devoted to "Classification of Mammals," a tabular arrangement which shows one of the recognized groupings in the scientific scheme of classification down to the families.

One hundred and forty-six small pen-and-ink sketches and half tones, most of which are good, picture both little-known and well-known mammals mentioned in the text.

This book will undoubtedly prove to be so useful that it should go through several editions, which will give an opportunity for adding other names and descriptions and making such minor changes and corrections as may be found desirable in the light of very critical use of the book. It is definitely a worth-while publication and brings to mind the very useful *Dictionary of Birds,* by Newton, although this work is much smaller.

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MICROBIOLOGY

Evaluation of Chemotherapeutic Agents. Colin M. MacLeod, Ed. xii + 205 pp. $4.00. Columbia University Press. New York.

FOR the past three years the Section on Microbiology of the New York Academy of Medicine has been annually holding symposia on subjects of current interest in the field of microbiology. In 1948, in the symposium of that year, the "Evaluation of Chemotherapeutic Agents" was discussed by fourteen outstanding authorities in the field; these discussions comprise the book under review.

Without doubt, the papers collected together in this volume represent carefully studied and authoritative opinion in a field that is rapidly expanding. Among the subjects critically discussed are the significance of drug concentration, blood levels on renal clearances, the binding effect of proteins, the problem of microbial resistance, defense mechanisms, localized infections, the nature of local lesions, chemoprophylaxis antimalarial drugs, rickettsial diseases, viral infections, and the chemotherapy of cancer. The whole field of the evaluation of chemo- and antibiotic therapy is brought into focus in this volume. Every student in this field and every clinician who employs chemo- and antibiotic agents in his practice would profit by reading these discourses.

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CORRESPONDENCE

IN DEFENSE OF LYSENKO

Robert C. Cook's article, "Walpurgis Week in the Soviet Union," in the June issue, is a remarkable performance. In his defense of democracy, Mr. Cook denounces all who dare disagree with him as givers of aid and comfort to the enemy, and makes it clear that anyone so unwise as to reply is to be considered in advance an enemy of democracy. Despite this danger—and it is a serious one—it would be spineless to permit his article to go unanswered. There is no question here of any unfairness to the Soviet Union, but merely of the fact that his emotional discussion is a distinct disservice to science.

Mr. Cook, with considerable support from the hysteria-provoking headlines of our press, evokes a picture of terror against scientists in the Soviet Union. He is a little careless, however, about details. On page 368, for instance, Vavilov, Lysenko's opponent, "died mysteriously in Siberia in 1942." On page 369, research has added to our knowledge, and it is stated flatly that Vavilov was "liquidated in 1942."

Mr. Cook can give no references to confirmed reports of Vavilov's "liquidation." He can refer only to rumors, made venerable by continuous repetition. Back in the thirties, Vavilov was so often killed or imprisoned—in American newspapers—that he sent an indignant denial to the American press, which he accused of using his pretended persecution merely as a stick with which to beat the Soviet Union. In 1942, when his death was reported, he was supposed to have been liquidated mysteriously in Leningrad; the site of liquidation was later shifted to Siberia, in the minds of American readers a much more sinister locale.

Mr. Cook does not trouble to explain why Vavilov should be done away with in 1942, a year which saw the Soviet Union ready to grasp the hand of any temporary ally who offered the slightest help—even of so determined a long-time enemy as Winston Churchill. Nor does it strike Mr. Cook as odd that N. Vavilov should be liquidated and his brother, who shared many of his views, should be elected head of the Academy of Sciences.

Mr. Cook notes a report that "Schmalhausen, Dubinin, and Orbeli have been liquidated." He adds, "This has no confirmation." But once the rumor is spread, the damage is done, the picture of terror evoked. (It it well to remember, however, that such liquidations are reversible. After many liquidations, Litvinov returned from the grave to become wartime ambassador to the United States. And on September 1, 1947, in different pages of the Washington *Evening Star,* General Zhukov was given an important assignment and liquidated on the same day.)

The fact that a discussion of "liquidation" occupies so large a portion of Mr. Cook's article is no accident. Such a discussion serves the purpose of shifting the ground, of preventing judgment of scientific principles and theories on their merits, of persuading American scientists that Russians do not hold the opinions they express, but have been terrorized into accepting them—and therefore that the opinions are not worthy of serious consideration.

Most Soviet biologists once shared Mr. Cook's low estimate of Lysenko. Like him, they ridiculed Lysenko's views, called them unscientific, and refused, taking advantage of prestige and official positions, to appoint Lysenko's supporters to research and teaching posts.

The Soviet government did not, for a long time, intervene in Lysenko's behalf. It adopted, instead, the position of mediator, and in 1939 arranged a conference on genetics and selection at which all the participants were allowed complete freedom of discussion. At the conference, the philosopher M. Mitin, who more than anyone else expressed the attitude of the government, praised both Vavilov and Lysenko for their achievements (several years after Vavilov's original "liquidation") and criticized them both for their shortcomings. Vavilov, he pointed out, tended to separate his theory from practice; Lysenko and his followers, he said, were dogmatic and made exaggerated claims. The conference ended on the note that both sides were working for the advancement of biology, and that actual results would decide between them.

Lysenko himself obtained products which orthodox genetics could not explain and, in fact, claimed could not be obtained by his methods. Practical methods he recommended were tremendously useful to Soviet agriculture, before, during, and after the war, all through a period when agricultural production was a matter of life and death.

The Soviet government was impressed by these facts; Mr. Cook is not. He sneers at Lysenko as only "a practical plant breeder," just as he sneers at Burbank as "capitalism's Michurin," who had "an inflated ego, a flair for weird statements, and a contempt for plodding experiments." That Burbank also obtained results that his more orthodox rivals, with their uninflated egos and their unweird statements, could *not* obtain, strikes him as merely a matter of " 'a green thumb'."

The orthodox geneticists were, on the whole, in control of the universities and the more academic scientific institutes. Lysenko's supporters, on the other hand, were recruited mainly from the agricultural institutes. The struggle between the two groups came to a head when the latter found their experiments limited by the authority of the orthodox geneticists.

In discussing the resulting conference, Mr. Cook notes that "the minority of Mendel-Morganists were frequently interrupted and ruthlessly heckled." This is true, but it hardly has the sinister significance Mr. Cook implies. Both sides were liable to interruption and heckling. During the 1939 conference, the shoe was mostly on the other foot, and Lysenko and his supporters were frequently interrupted by denials and shouts of contradiction from the audience. From our point of view this may be bad manners, but it is hardly sinister.

The government and the Communist Party did not interfere in the controversy. For those who are naïve enough to conclude from this that the government wanted to encourage freedom of expression, Mr. Cook has a more terrifying interpretation: the government wanted the assembled biologists to *commit themselves,* and then, after their words were recorded, Lysenko was given a

chance to "explode the bomb" by announcing that the Central Committee of the Communist Party had examined his report and approved it.

How the Soviet government can possibly escape from the dilemma in which Mr. Cook has placed it, I cannot see. If it announces its views beforehand, it is guilty of repressing freedom of speech; if it reserves its views, it is for the purpose of liquidating dissenters. Mr. Cook has it coming and going.

Mr. Cook is so far carried away by his emotions as to sneer at the "futility" of Soviet work in the Arctic (despite the very different opinions of such men as Stefansson). He even plunges rashly into the dangerous field of economics to support Varga, who had expressed the opinion that the United States might stave off an economic crisis for several years. Varga has changed his mind, and so of late have many of our home-grown economists. Mr. Cook has picked a bad time to defend Varga's opinions.

It is impossible to discuss every peculiar detail in Mr. Cook's report. But whatever American biologists think of Lysenko's views, they have a right to accept or reject them without being confused by tales of Soviet liquidation, or intimidated by the threat of their own liquidation from the faculties of American universities. They would do well to remember that even such American experimenters as Tracy Sonneborn have indicated that nuclear genes may not be the sole carriers of heredity (without, however, associating themselves with Lysenko's views).

In the December 1948 SCIENTIFIC MONTHLY, Jacob W. Gruber points out that the Cuviers, the Mayers, and the Virchows—among the most eminent men of science of their day—had "intellects imprisoned and imaginations shackled by hypotheses of their own making." Mayer, for instance, concluded that the Neanderthal skeleton was that of a "rickety Mongolian Cossack." And the author wonders "how many 'rickety Mongolian Cossacks' exist in the controversies and conclusions, the debates and deductions of today."

Our own eminent biologists may be as much prisoners of hypotheses of their own making as were Cuvier, Mayer, and Virchow. They would do well to acccept or reject Lysenko's views on their merits, and to guard against the invention of a liquidating Ukrainian peasant.

JOSEPH SAMACHSON

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MR. COOK REPLIES

In my forthcoming article in the *Journal of Heredity* I am citing the sources of statements made in THE SCIENTIFIC MONTHLY article. That Vavilov died late in 1942 or early in 1943 in the town of Magadan, in Eastern Siberia, was learned from reliable confidential sources. That his brother Sergei's presidency of the Soviet Academy of Science did not save him from some sinister fate is evidenced by one official Soviet datum. The invitations sent in June 1945 to attend the 220th Anniversary Celebration of the "Soviet" Academy of Sciences (founded by Peter the Great!) contained a list of living and of recently deceased members of the Soviet Academy of Sciences. The name of Nicolai Vavilov appears neither on the roster of the living nor of the dead. Hence it is a reasonable deduction that in 1945 he was not even a dead member in good standing. Ergo, something had happened to him. If Nicolai Vavilov is actually still on this earth, an interview with him by a person or persons who knew him prior to 1942 would cause Dr. Dobzhansky and me very cheerfully to eat every word we have said to the contrary.

That other distinguished Russian biologists have either lost their lives or been banished into an outer limbo of utter silence was a matter of common knowledge when Dr. Muller was in Russia before 1938. Since many recent firings have been officially recorded in *Pravda*, Samachson's demand that this part of the record be ignored as irrelevant is obviously absurd. Anyone who reads the recantations that followed the August 1948 meeting, and who followed the events recorded in *Pravda* during the succeeding weeks, knows the question of the *scientific validity* of Lysenko's views is not an issue. The biologist in Russia takes leave of all personal opinion and follows the party line, or he takes the consequences.

All the apologists for the Lysenko cult attempt to picture the conflict between Michurinism and Mendelism as a valid scientific controversy. This definitely and emphatically is not true. Lysenko's sacred "views" are not valid scientific hypotheses. His queer "Michurin teaching" *simply denies, and makes no attempt to account for, any of the abundantly demonstrated basic phenomena of Mendelism.* The position of the Lysenkoist is essentially that of a physicist who, boldly denying that stones fall if tossed from a window, builds on this brash allegation his own "private world" of views of physical reality.

Geneticists are not "prisoners of hypotheses" in their rejection of Michurinism. It is rather the Michurinists who are *prisoners of the facts.* Their false allegations ring them around with an impassable barrier of continued and repeated observation which prove their assumptions false or which permit of a simpler explanation. Saint Luther with his wheat "with the same specific gravity as granite," and Saint Trofim with his "thousands of graft hybrids" cannot outtalk reality. If Lysenko could account for the basic one-two-three's of genetics he might have a case, but that he has never done. It is significant that competent biologists seem unanimously agreed on this point. They are surprised that chemists like Samachson and Spitzer seem so intent on interpreting the basic facts of life for them.

ROBERT C. COOK

Journal of Heredity
Washington, D. C.

TO A YELLOWJACKET

I've learned a bit about you, in your gleaming black and gold
And of your fellow workers, whose multitude untold
Engage in every industry within your social mold.
I know your every synonym, your type and allotype.
I've watched you garner nectar from the fruit that is just ripe,
And malaxate your wood pulp, and tyloids gently wipe.
Hail to you, Vespine Beauty, with your eyes emarginate,
Yellow clypeus extending to your mandible dentate,
And your three ocelli shining from the vertex of your pate.
Hind angle of pronotum extending to the wing;
Your diploid gyne and ergates, your short-lived, haploid "king,"
But I've learned enough about you, through your carboxylic sting.

ALBRO T. GAUL

Brooklyn, New York